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THE EXTENT OF THE VISUAL AREA OF THE CORTEX IN MAN, AS DEDUCED FROM THE STUDY OF LAURA BRIDGMAN'S BRAIN.

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Some peculiarities of the cerebral cortex, in the case of Laura Bridgman, have already been described in this JOURNAL ([1, 2]).

It was there pointed out that the three localities examined within the occipital lobe had the cortex thinner in the right hemisphere. The optic nerve and tract belonging to the left eye were the thinner, and in the right cuneus, the gyri were irregular. These facts all pointed to a greater disturbance affecting the right occipital lobe. Since in the left eye vision was completely lost at the age of two years, while it persisted up to the eighth year in the right, the thinness of the cortex on the right side was explained by the earlier loss of vision in the left eye, a lesion which was assumed to have arrested its development. Since the completion of the papers mentioned above, a further consideration of the case suggested that this brain might be used to determine the extent of the visual area in man. In following out this suggestion it has been assumed: (1) That the thinning of the cortex was due to an arrest of development; (2) that this thinning would extend over the entire visual area; (3) that, in the regions compared, the disturbance in vision was the principal influence acting to arrest unequally the growth of the cortex;

(4) that the cortex would be most thinned upon the side of the brain opposite to the eye and nerve most affected; (5) that the visual area in the normal brain gradually merges into the surrounding areas, and that hence the cortex on the outer limits of this area would, in this case, be but slightly different on the two sides.

The observations on which my conclusions are based were made in the following manner:

On a model of the brain, a boundary line was marked off as follows: Beginning at the mantle-edge on the dorsal surface the boundary passed laterad from the mesal end of the sulcus lying next caudad to the superior retrocentral sulcus, to the dorsal end of the ascending ramus of the first temporal sulcus, following this ventrad to its junction with the first temporal sulcus, and thence took the shortest path over the lateral and ventral surfaces, to the most cephalic end of the calcarine fissure, and there passed dorsad to join the mantle-edge at the point of departure. There was thus cut off a pyramidal portion of the hemisphere, with the plane of its base determined by this boundary, and having the occipital pole for its apex.

For the purpose of comparison, this surface was first divided into six areas, and ultimately, by subdivision, into ten areas. The following is a description of the areas.

I. The superior-parietal area. Bounded mesad by the mantle-edge, laterad by the interparietal sulcus, cephalad by the sulcus next caudad to the superior retrocentral sulcus, caudad by the anterior occipital sulcus.

I. was subdivided into a cephalic portion, I. (a), lying cephalad to the cephalic stipe of the interparietal sulcus, and a caudal portion, I. (b), lying caudad to the same.

II. The area of the angular gyrus. Bounded mesad by the interparietal sulcus, laterad by an arbitrary line uniting the junction of the first temporal and its ascending ramus to the transverse occipital sulcus, cephalad by the ascending ramus of the first temporal sulcus, and caudad by the anterior occipital sulcus.

II. was subdivided into a cephalic portion, II. (a), lying cephalad to the ascending ramus of the second temporal sulcus and a caudal portion, II. (b), lying caudad to the same.

III. Area of the cuneus. Bounded dorso-cephalad by the parieto-occipital sulcus, ventrad by the calcarine fissure, and caudad by the mantle-edge.

IV. The area of the occipital pole. Bounded mesad by the mantle-edge which separates it from the cuneus, laterad and cephalad by the anterior occipital sulcus, caudad by the

lateral occipital sulcus, and an arbitrary line joining the mesal end of the latter with the calcarine fissure.

IV. was subdivided into a lateral portion, IV. (a), lying laterad to a secondary sulcus which runs through the middle of this area, parallel to the mantle-edge, and a mesal portion, IV. (b), lying mesad to the same.

V. The praecuneal area. This includes that part of the praecuneus which lies cephalad to the parieto-occipital sulcus, and caudad to the boundary line.

VI. The ventral area. Bounded mesad by the calcarine fissure, laterad and caudad by the lateral occipital sulcus and its arbitrary continuation to the junction of the first temporal sulcus with its ascending ramus, cephalad by the base line.

VI. was subdivided into a mesal portion, VI. (a), lying between the calcarine fissure and the fourth temporal sulcus, including, however, only the dorsal two-thirds of the gyrus lingualis, and a lateral portion, VI. (b), lying laterad to VI. (a).

The brain itself was now compared with the model. The portion corresponding to each one of the ten areas just enumerated was cut into a number of blocks, and the position of each block was carefully marked on the model. The block was then mounted and the thickness of the cortex determined. There were sixty-two such blocks for each hemisphere.

The thickness of the cortex was determined in the following manner: The block was imbeded in celloidin and cut into sections about .05 mm. thick, stained with van Gieson's Picro-Fuchsin and mounted in the usual way.

By this stain the gray matter was colored a much deeper red than the white and thus well differentiated from it. The thickness of the cortical layer was measured with an eye-piece micrometer, each division of which had the value of .067 mm.

The following is the detail of the method of measurement: By means of a camera lucida each specimen was drawn on a card, being enlarged about six diameters. The actual length of the cortex in each specimen was next obtained by direct measurement. The thickness of the cortex was measured at the bottoms and sides of the sulci and at the summits of the gyri. Whenever any of these localities was sufficiently extended, more than one measurement was taken. In general the measurements of the thickness of the cortex were made at intervals of 5 mm.

Each measurement was then recorded at the corresponding point on the drawing of the specimen, and by this means any observation could be easily verified.

The above was all done by my assistant who was not informed of the purpose of the investigation.

The next step was to cover all the labels, both on the specimens and the drawings. The drawings were then shuffled and the specimens arranged in the final order in which the drawings stood. In this way the specimens from the right and left sides were mixed together and all order among them destroyed. All the measurements were then repeated by myself. The figures thus obtained were final and have not been altered.

As a result there was obtained for each section a length of cortex and a certain number of measurements giving the thickness of the cortex at about equal intervals of 5 mm. The average of the measurement for thickness multiplied into the length represented the area of the cross-section of the cortex in the given block. In order, however, to compare these areas with one another, it was necessary that the length of cortex should be the same on both sides. As this was rarely, if ever actually the case, the adjustment was made by always choosing as a norm the length of that side where the cortex was shortest. In comparing symmetrical points the difference in the area is thus rendered dependent upon the average thickness of the cortex alone.

In making up the tables from these observations it has to be kept in mind that if from one block in a certain area we have five or ten times the length of cortex that is obtained from its neighbor, that then the average derived from the measurements of this longer cortex applies to five or ten times as much of the region from which it is taken and hence should enter into the sum from which the average is derived five or ten times to its neighbor's once. In making the tables, therefore, the average thickness of the cortex for each block is multiplied by the length of cortex to which it applies. The areas thus obtained are added together and the sum divided by the number representing the sum of the several lengths of cortex. The quotient represents the average thickness of the cortex for the area in question, the thickness as determined for each block having thus been allowed its proportionate weight. Since the length of the cortex was very different in the different blocks the length in that block which had the shortest cortex was taken as a standard. The minimal length was just 5 mm. This length of 5 was taken as a unit, and all the other lengths have been written in the tables as multiples of this unit. In other words those numbers represent the true length of the cortex divided by 5. Since the reduced lengths enter as factors into the columns in which the areas are given, the figures there also represent

the true areas divided by 5. With this explanation we present the first table.

TABLE I.

To show the average thickness of the cortex in the several subdivisions, with the absolute difference between the two sides and thickness of right side in percent.

AREA.	No.	Number of Blocks.	Length of Cortex divided by 5.	Area of Cortex in section, divided by 5.		Average thickness in mm.		Left side standard.		
								Absolute difference in mm.	Percentage thickness of the right side.	
Name.				Left.	Right.	Left.	R'h't			
Superior parietal.	I. a.	2	9.1	24.68	24.49	2.71	2.69	+.02	99.2	excluded.
Superior parietal.	I. b.	3	12.5	37.27	34.32	2.98	2.74	+.24	91.9	
Angular gyrus.	II. a.	5	20.5	58.09	56.97	2.83	2.78	+.05	98.2	
Angular gyrus.	II. b.	6	20.2	61.15	53.30	3.02	2.63	+.39	87.0	
Cuneus.	III.	11	38.7	82.69	79.13	2.13	2.04	+.09	95.7	
Occipital pole.	IV. a.	6	19.1	42.79	41.83	2.24	2.19	+.05	97.7	
Occipital pole.	IV. b.	3	14.8	32.76	31.93	2.21	2.15	+.06	97.2	
Praecuneus.	V.	4	19.6	53.56	58.97	2.73	3.00	—.27	109.8	excluded.
Meso-Ventral.	VI. a.	6	16.4	37.50	33.41	2.28	2.03	+.25	89.0	
Ventral.	VI. b.	17	60.1	140.82	148.37	2.34	2.46	—.12	105.1	excluded.

EXPLANATION OF TABLE I.

In this table are entered the numbers for the ten subdivisions. The numbers for the larger areas, e. g. I., etc., are not entered because in the presence of the subdivisions they are superfluous. They can, moreover, be obtained in any case by summing the numbers given for the component subdivisions.

Taking the columns of the table from left to right we have the following data:

(1) Name of area, (2) numerical designation of the subdivision, (3) the number of blocks cut from the subdivision, (4) the total length of the cortex in said blocks, divided by 5, as previously explained. This length has been adjusted so

that it is equal on both sides of the brain. The product of this last number into the number representing the average thickness of the cortex in this subdivision, first for the (5) left, then for the (6) right side. The number in millimeters, representing the average thickness of the cortex in the given subdivision, first for the (7) left, then for the (8) right side. The larger number of each pair in these two columns is printed in heavy faced type. (9) The absolute difference in millimeters between the pairs of figures in the last two columns, showing by how much the number for the left side exceeds that for the right. (10) The thickness of the right side expressed as a percentage. The thickness of the left side being taken as a standard i. e., equal to 100%. (11) Finally, "excluded" is put after each subdivision which can not be considered as part of the visual area.

The numbers for each subdivision as entered in Table I. have been derived from detail tables, giving the length of cortex and average thickness of the same for each block belonging to the subdivision. These detail tables would be worth publishing only in case they were accompanied by figures showing exactly the point at which each section was taken. At present that is not practicable, hence they are not given.

An examination of Table I. shows that in two subdivisions the cortex on the right side is thicker than on the left. Since we are determining the extent of only that cortex on the right side which is thinner, these two cases, V. and VI. (b), are to be at once excluded. In the remaining eight cases the thickness of the right side is from 99.2% to 87.0% of that of the left. The next question, therefore, is what difference may be considered significant for our purpose.

To this end we must determine first, what variations occur in the thickness of the two sides of normal brains.

I have determined the relative thickness of the occipital cortex in the brains of six males and three females, sampling each hemisphere at three points ([2, p. 62]).

Conti([3]) has determined the same for seven males and four females. The figures apply to his post-rolandic region, namely all that which lies behind a vertical plain passing through the mesal end of the rolandic fissure.

Franceschi's([4]) observations were made on ten male and ten female brains and appear to apply to the occipital lobe of Ecker.

In general, then, the figures obtained by Conti and Franceschi are comparable with those which I have obtained from these last measurements of the Bridgman brain. The averages from the controls measured by myself are taken from

three localities only, and may, therefore, be expected to show greater variations than the figures of the Italian observers. The following, Table II., shows the thickness of the cortex in the left and right occipital lobes of the normal brain, giving the thickness of the right side as a percentage of that for the left.

TABLE II.

			Males.				Females.			
Authority.	Date	Locality examined.	No. of Brains.	Average thickness of Cortex in mm.		Percentage thickness of right side.	No. of Brains.	Average thickness of Cortex in mm.		Percentage thickness of right side.
				Left.	Right.			Left.	Right.	
Conti.	1884	Post Rolandic Region.	7	2.08	2.06	99.0	4	2.03	2.03	100.0
Franceschi.	1886	Occipital Lobe.	10	2.28	2.29	100.4	10	2.16	2.14	99.0
Donaldson.	1891	Three points in Occipital Lobe.	6	2.65	2.62	98.8	3	2.48	2.54	102.4
						Average, 99.5				Average, 99.8

Table II. shows the greatest difference between the right and left sides in the case of the six normal males examined by myself, the right side being the thinner, 98.8% of the left. In the females the right side is less thin, averaging for the seventeen cases 99.8%, while for the twenty-three males it is 99.5%.

Taking the greatest difference found, 98.8%, and applying it to Table I. we exclude area I. (a), since there the right side differs from the left less than in the case of the normal males just cited.

The other differences shown in Table I. we may now regard as significant and employ them for plotting the visual area. See Plate I.

According to these figures the outline of the visual area is the following: Commencing where the cephalic stipe of the interparietal sulcus cuts the mantle-edge and passing latero-cephalad along the latter to its junction with the inferior retrocentral sulcus, the boundary then takes the shortest line

to the ascending ramus of the first temporal sulcus, following this to its union with the sulcus, from here the shortest line to the lateral occipital sulcus from the mesal end of which an arbitrary line turns toward the fourth temporal sulcus, running parallel to this sulcus it cuts the gyrus lingualis so as to leave the ventral third of the latter in connection with the fourth temporal sulcus and continues to a point just ventrad of the cephalic end of the calcarine fissure, which it joins by an arbitrary line running dorsad, it then passes caudad along the calcarine fissure to the junction of the same with the parieto-occipital sulcus, and finally along this sulcus to the mantle-edge, then cephalad along the latter to the point of departure.

Such are the limits of visual area, as determined by this method, in the right hemisphere of the Bridgman brain.

All previous direct determinations of this area in man have been by the method of limited lesions, and I find that my outline follows so closely that given by Gowers([5]) in his diagram and obtained by the latter method, that I venture to think that the two methods lead to almost identical results and hence mutually support one another.

The visual area here outlined can, however, be examined in detail with advantage. The areas of the cuneus and that of the occipital pole [III., IV. (a) and IV. (b)] show the cortex on the right side only slightly thinner than on the left. The areas immediately around those just mentioned, viz., I. (b), II. (b) and VI. (a), show the greatest thinning on the right side. Beyond this second series we have but one other area, II. (a), in which the difference is again small.

The theoretical explanation for this relation of things is the following: The cuneus and the occipital pole form the more fundamental portion of the visual area, hence would be earlier developed and more resistant to disturbing influences. If such were the case the differential effect of the lesion would be less evident in this region because the growth was more nearly complete at the date of the injury. Moreover, both eyes are represented on each side in the areas of the cuneus and occipital pole, and hence the loss of sight in the left eye would have produced some arrest on the left side, though we should expect the arrest to be decidedly less than on the right. So much by way of explaining why the regions of the cuneus and occipital pole do not show greater differences in the two sides.

To explain why the surrounding region does show greater differences, I assume, first, that this region reaches complete development later, and would, therefore, be more affected under these conditions. Where the greatest difference occurs,

namely, in the caudal portion of the angular gyrus, II. (b). We have a region which is held by some authors to be in connection with the opposite eye mainly, and hence we have here two conditions which would induce the greatest difference in growth between the two sides. In the outermost area, II. (a), I look upon the visual representations as decreasing, hence the loss of vision would produce a less evident arrest here.

It is hardly desirable to further dwell on these explanations, since, in part at least, they can be tested, and it can thus be determined whether they are valuable.

In this connection there is one remaining observation to be noted. The mounted specimens of the cortex all show to the naked eye a clear stratification due to a light line. If the specimens are sorted according to the distinctness of the light line, disregarding the labels, and we put by themselves those in which the line is clear and sharp, it is found that we have all the samples of the cortex from the cuneus, the occipital pole, and also from the affected part of the gyrus lingualis.

It thus happens that the line of Baillarger or of Gennari, as it has been rechristened by Obersteiner, is co-extensive in this brain with portions of the visual area, which I have called fundamental, but also runs over to the gyrus lingualis, which can not be included in that part for the reasons above given. So evident a structural peculiarity must have some physiological significance, but this case as it stands, does not show what that is.

We conclude then that in this single brain we have the entire visual area marked out. This area includes the cuneus and angular gyrus, but does not pass on to the ventral surface. The thinning of the cortex is not the same throughout the visual area, but is small in the cuneus and occipital pole, large in the areas immediately surrounding it, and, finally, small again in the most outlying portions. The explanation which I have offered for this, is the stage of development in which the various portions of the cortex were found at the time vision was lost, and the degree to which each eye is represented in several portions of both areas. Finally, Gennari's line almost coincides with the fundamental portions of the areas, but oversteps it so far as to take in the dorsal portions of the gyrus lingualis, and thus, at present, cannot be explained as a peculiar character of the fundamental portion.

The idea involved in this investigation is not novel. The observations of Huguenin(6), Burckhardt(7), Mickle(8) and Mills(9) were based upon it, but this, I think, is the first time that advantage has been taken of an opportunity to apply it in detail.

To myself the point of most interest is that, if these conclusions are warranted, we have now, through the early destruction of sense-organs and the subsequent examination of the cortex, a means of experimentally determining in animals the limits of the several sensory areas. For the feasibility of this plan, the experiments of v. Gudden and his school already offer some indirect support.

Explanation of Plate I.

This plate has been made in the following way: On the right side the caudal portion of the right hemisphere is represented in some detail. There are three views: Fig. 1, lateral; Fig. 2, caudal; Fig. 3, mesal. The reversed outline of this same portion is represented on the left side of the plate. The boundary of the portion taken is marked by a heavy line. On the left side the limits of the subdivisions are all marked by broken lines, and within each subdivision is given its numerical designation and the average thickness of the cortex, in millimeters. These are repeated on the right side, but the broken lines are omitted. As will be seen, in certain cases, some of these data are omitted. No ventral view is given because the cortex there is excluded. The excluded portions, so far as shown, are left white, but on the right side the less affected subdivisions are hatched with a single line, while the more affected are so indicated by a doubly hatched line. The hatched portion on the right side, therefore, represents the visual area as determined.

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2.	1891.	Ibid. *American Journal of Psychology*, Vol. IV, No. 2.
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5.	1888.	Gowers, W. R. A manual of diseases of the nervous system. Vol. II. London, 1888. Vide p. 19.

PLATE I.

Showing three aspects of the occipital lobe.

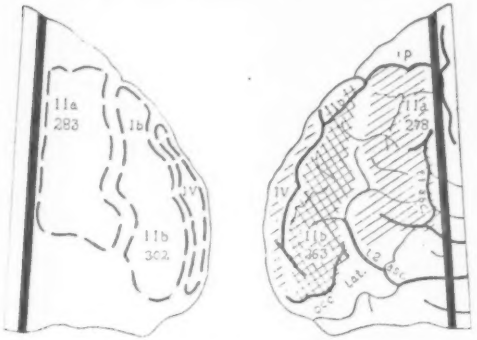

FIG. 1. Lateral aspect.

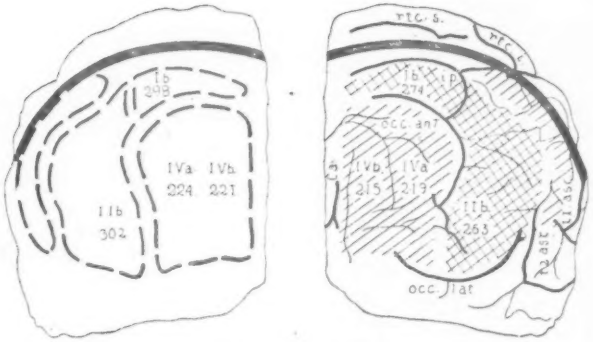

FIG. 2. Caudal aspect.

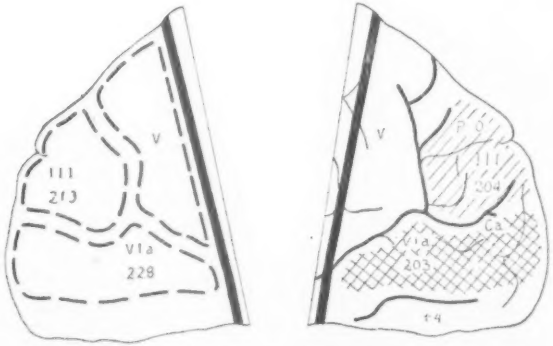

FIG. 3. Mesal aspect.

6. 1878. HUGUENIN. Ein Beitrag zur Physiologie der Grosshirnrinde. *Correspondenzblatt f. Schweizer Aerzte.* 1878. Nr. 22.

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SOME INFLUENCES WHICH AFFECT THE RAPIDITY OF VOLUNTARY MOVEMENTS.[1]

By F. B. DRESSLAR, Fellow in Psychology in Clark University.

The purpose of this research was (1) to find a convenient and accurate method of recording and timing rapid voluntary movements, and (2) to determine some of the conditions which influence the rate of such movements. The experiments were limited to a study of the rate at which one can tap on a Morse key. Three hundred taps were made as rapidly as possible at each test, and the condition of the subject noted. During the course of the experiments over 400 tests were made, involving more than 120,000 taps.

Apparatus. The apparatus used consisted of a kymograph with the revolving drum set perpendicularly, and covered with smoked paper. On a standard was fastened an electro-magnet which was put in a circuit alternately made and broken by a clock. Attached to the armature of this magnet was a projecting metal point which registered the seconds on the revolving drum. On the same standard was fastened a common clock movement, the escapement of which was alternately raised by an electro-magnet at one end, and by a resisting spring at the other, according as the electric circuit was made or broken by a Morse key. Thus with each tap on the key the escapement wheel was permitted to move one notch. To prevent jars this key was placed on an adjacent table, but near enough for the operator to manage the kymograph. Upon this key the taps recorded were made. In order to register the taps on the slowly moving drum, a silk cord was passed once around a small pulley substituted for the second hand of the clock. One end of this cord was made fast to a wire bearing a fixed writing point, the other to a counter-weight. The wire was held in a perpendicular position by passing it through two brass strips extending out from the

[1] The work was done under the direction of Dr. Warren P. Lombard, Assistant Professor of Physiology, to whom I am indebted for many suggestions in determining methods for work, and for help in devising apparatus.

board on which the clock was fastened. The writing point was fixed on the wire between these strips and the amplitude of its movement made correspond exactly to 300 taps on the key, by adjusting a wooden clasp on the upper strip. When the drum was moving, the line traced by the writing point formed an angle with the abscissa which varied according to the rapidity of the taps on the key.

To determine the time required to make 300 taps, perpendiculars were drawn to the seconds line from the points where the writing point began to rise and where it reached the upper limit of its motion. The seconds comprehended between these perpendiculars gave the time.

The following is an example of a normal record.

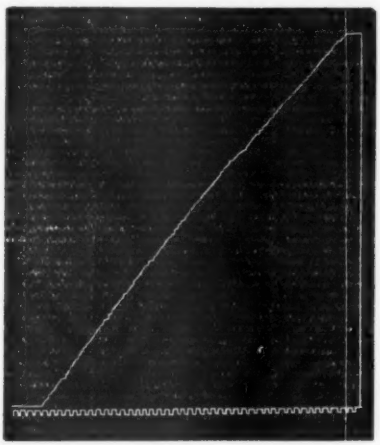


FIG. 1.

Method of Work. From 8 a. m. to 6 p. m. six tests of 300 taps each were made; one every two hours. The usual working hours of the day were thus covered by the experiments, and sufficient time intervened between the records for the subject to recover from any possible fatigue. Three hundred taps were chosen because it was thought that after practice this number would not be fatiguing[1] and at the

[1] It was noticed after the tests had all been made that many of the curves showed a decreasing rate towards the end of the 300 taps and thus gave evidence of incipient fatigue, though all sensations of fatigue had ceased with the first few days of the work. By actual count out of one hundred curves taken at random from the last month of work, seventy showed the effect of this unconscious fatigue. This strengthens the belief that the method used may prove useful as a mild but sensitive clinical test.

same time sufficiently great to show any fluctuation in the normal rate. The course of the experiments furnished no evidence that this number was not well chosen.

The fore-arm rested on a firm support and the tapping movements were as far as possible confined to the wrist. Throughout these experiments the subject gave his whole attention to the work, and made the 300 taps with the greatest possible rapidity. The condition of the operator was carefully recorded beneath each record, and especial attention given to any circumstance which it was thought might influence the rapidity of the taps.

Normal rate of voluntary taps. Table I, column 1, shows that the average rate attained for 300 taps for the time there recorded was 8.5 taps per second. Column 2 should not be included in the average, because during this period many conditions were introduced that more or less affected the normal rate. The most rapid rate attained for 300 taps for any single record was 10 taps per second. Even this last rate could be excelled for a short time. From 30 tests made at the beginning and near the end of the work, the rate for the left hand was found to be 5.3 taps per second.

TABLE I.

	1.							2.						
	No. of seconds required to make 300 taps.													
DATE.	8 A.	10 A.	12 M.	2 P.	4 P.	6 P.	DATE.	8 A.	10 A.	12 M.	2 P.	4 P.	6 P.
Feb. 3	37	38	36	35	34.5	36	Mr. 10	44.25	37	38	39	34	36
" 4	37	38	33.5	35.5	32.5	36	" 11	37	34.5	38	42	32.5	36
" 5	40	35.5	34.5	35.25	34.5	34.5	" 12	43	41	36	40.25	36	40
" 8	39.5	34	33.25	36	33.5	34.5	" 14	42	43	38.5	43	40	43.25
" 9	37	32.5	34	36.5	37		" 15	44	39	39	42	39	46
" 10	39.75	37	34.75	35	34	33	" 16	45	38	38.5	41	34	43
" 11	38	36.5	36.5	34.5	36	37	" 17	42	39.5	40	41.75	39	40
" 12	40.5	37.5	37	38	33	36	" 18	43.5	40	44	40	37	38
" 13	37	38.5	37.5	39.5	34.5	37	" 19	39	37.5	36.5	40	36	38
" 15	38.75	31.25	31.5	30	30.5	32	" 21	40.25	40	39	40	36	42
" 16	33.5		33.5	33.5	31.25	38.25	" 22	41	41	37.5	40	36.25	38
" 17	36.5	33	32	32.75	32	34	" 23	45	38	39	37.25	37	39
" 18	36	35.25	36.5	37.5	33	33	" 24	40	43.75	41	40.5	38	48
" 19	39	34.5	33.5	37.25	32	35	" 25	55	47.5	37.25	41.25	36.5	37
" 20	37	35	35	36.5	34.5	33	" 26	40.5	40.5	40	40	38.5	39
" 22	40.75	36	38.5	33			" 28	42.5	38.5	42.5	43	41	41.5
Aver.	37.8	35.4	34.6	35.5	33.5	35.1							

General average 35.3.

The rapidity with which the taps were made, at first suggested that the movements were not voluntary in the stricter sense, but more in the nature of tremor. To test this, the contacts of the Morse key produced by normal taps, were recorded on a revolving drum by a Deprèz signal, and each movement timed by a tuning fork of 100 V. D. These were compared with records taken in the same way for tremor of the wrist, and also of the fore-arm. All attempts to produce tremor of the wrist resulted in lateral movements instead of the up and down movements used in making the regular taps.

This fact, together with the greater rapidity of the tremor attained in both cases, seems to leave no doubt that the taps made were separate voluntary movements. Figure (2) shows the contrast (A, B) between the usual taps and tremor of the fore-arm, and (A', B') between the usual taps and tremor of the wrist. It will be seen from this figure that during short intervals the subject could make 10.5 taps per second, while the rate of the oscillations of the fore-arm during tremor was 12.2 per second, and for lateral tremor of the wrist 12.9 per second.

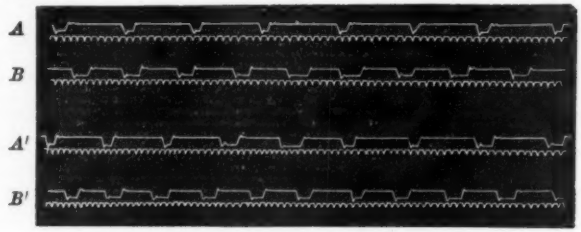


FIG. 2.

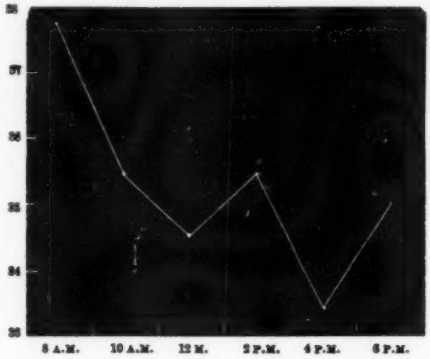
A and A' = Voluntary taps.
B and B' = Tremor; B of fore-arm, and B′ of wrist.

Voluntary Movements.			Tremor.		
Time in hundredths of a second.			Time in hundredths of a second.		
Line	No.	Time.	Line.	No.	Time.
A	1	9.8	B	1	8
"	2	9.2	"	2	8.5
"	3	9	"	3	7.75
"	4	10.5	"	4	8.8
"	5	9.6	"	5	7.8
"	6	8.1	"	6	8.4
"	7	10	"	7	7.5
A′	1	9.4	"	8	8.75
"	2	10.2	B′	1	6.8
"	3	8.9	"	2	8.2
"	4	9	"	3	8.3
"	5	9.5	"	4	7.8
"	6	10	"	5	7.5
"	7	9.5	"	6	8
"	8	10	"	7	6.7
			"	8	8
Average.		9.5	"	9	8
			"	10	8
			Average.		7.9

Daily rhythm. After six weeks of work it was seen that the daily records indicated a daily rhythm and, when no varying influences were introduced, the records for the corresponding hours of each day were quite similar. The averages of the time required to make 300 taps at the different hours on the days given in Table I, column 1, were as follows :

Time of day.	8 A. M.	10 A. M.	12 M.	2 P. M.	4 P. M.	6 P. M.
Time in seconds.	37.8	35.5	34.6	35.5	33.5	35.1

The following characteristic curve is obtained from these averages. This curve would be much less significant did it not correspond very closely in shape to the normal daily curve.



NOTE.—Distance along the abscissae represents time of day; along the ordinates the number of seconds required to make 300 taps.

The shape of this curve was unexpected, for it was naturally thought that the greatest rapidity would be attained sometime during the forenoon. It will be shown later that the rate at which it was possible to tap was increased by mental excitement or activity, This fact is in harmony with the daily rhythm. The activity of the central nervous system probably increased during the hours of work from 8 to 12, lessened during the noon hour of relaxation, increased again until 4 o'clock in the afternoon, and then, the chief work of the day being over, again decreased. The close correspondence of this curve to the daily programme of work for two years previous is suggestive in the direction of habit; these were spent in public school work with a daily programme beginning at 8 a. m. and closing at 4 p. m., with an hour and a half intermission at noon.

It is probable also, that corresponding changes in the tonus of the muscles of the body, accompanied the changes of central activity, and this increased muscular tension mechani-

cally favored the rapidity of the movements. The decreased rate at 2 p. m. cannot be accounted for by the walk to and from meals, as special experiments showed that the rate was not affected by such short walks. The rise at 6 p. m. may partly be due to fatigue, but it is thought to be chiefly due to an unconscious relaxation, as the chief work of the day ended at 4 p. m. By a careful study of the barometric charts extending over the whole series of experiments, it was found that no effect of atmospheric pressure could be detected.

Effect of Muscular Exercise. After almost two months of work it was noticed that irregularities were introduced into the daily curve when a vigorous walk had been taken. To test this, walks were purposely taken between different periods of work for a series of days; the following table shows the average results; these averages would be less significant, did not the individual cases show every time a decreased rate after the walk:

Before Walking.		After Walking.		
Time of Day.	Time in sec. for 300 taps.	Time of Day.	Time in sec. for 300 taps.	Difference.
8 a. m.	41.75	10 a. m.	43.75	2 sec.
10 a. m.	39	12 m.	43.5	4.5 "
12 m.	36.5	2 p. m.	44.5	8.5 "
2 p. m.	39	4 p. m.	44	5 "
4 p. m.	39	6 p. m.	46	7 "

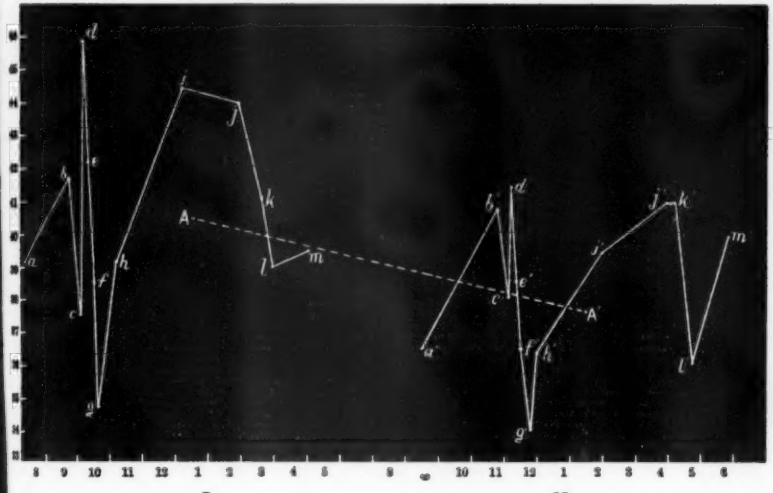
This falling off in rate after a walk is probably due to (a) general fatigue and (b) mental relaxation induced by the walk, because a walk toward the close of the day when the normal rate was the highest, produced more marked effect than a similar walk in the morning.

Effect of Vigorous Mental Exercise. It was noticed by a comparison of records and the daily journal, that strong mental concentration, especially if accompanied with the excitement of interest, was favorable to rapidity of movement. To test this further, records were taken immediately before and after sorting into heaps as rapidly as possible 80 cards of 10 different kinds. Several packs were rapidly distributed with a few seconds rest between, until it required the utmost concentration to fix and keep the position of the different kinds

in mind. The exercise is somewhat exciting and to make the best time it requires the most undivided attention. The following table gives the time in seconds required for making 300 taps before and after such experiments:

Before Sorting Cards.			After Sorting Cards.		
Date.	Time of Day.	Time in sec. for 300 taps.	Time of Day.	Time in sec. for 300 taps.	
Apr. 5	3.00 p. m.	44	4.00 p. m.	41	−3
" 8	10.45 a. m.	34.75	11.05 a. m.	38	+3¼
" 8	5.05 p. m.	39.5	5.45 p. m.	39	−½
" 9	8.10 a. m.	39	8.30 a. m.	36.5	−2½
" 9	11.10 a. m.	39.25	12.00 m.	36.5	−2¾
" 11	10.10 a. m.	46	10.45 a. m.	40.75	−5¼
" 11	3.40 p. m.	41	4.05 p. m.	41	0
" 12	4.00 p. m.	39	4.45 p. m.	36	−3
" 13	10.35 a. m.	38.5	11.20 a. m.	38.5	0
" 14	1.15 p. m.	44.5	2.00 p. m.	39.5	−5
" 15	9.45 a. m.	41.75	11.05 a. m.	41.5	−¼
" 16	10.05 a. m.	37.5	11.45 a. m.	34	−3½
" 18	10.25 a. m.	42.25	11.30 a. m.	36.5	−5¾

The similarity of the curves for these figures is very striking, and they also show the marked difference in rate.



I. II.

Before Sorting Cards. After Sorting Cards.

NOTE.—Points a, b, c, d, etc., curve I, represent tests before sorting cards, and a', b', c', d', etc., curve II. corresponding tests after sorting cards. The dotted line, $A\,A'$, connects the points representing the averages before and after sorting the cards.

After studying the variations caused by such severe mental work, and taking into the account that vigorous physical exercise had produced the opposite effect, it seemed probable that the increase in rate was due to increased central activity and the unconscious tension of the muscles attending this central excitation. This view was somewhat strengthened, when a study of the whole series of experiments revealed the fact that an increased rate had accompanied mental excitement. For example, the rate was increased after reading an interesting unexpected letter, after the announcement of a distinguished visitor to inspect the work, and just before reading a paper before the psychological seminary. While these observations are too few to base upon them definite statements, they are of value, because such conditions cannot be manufactured at will.

From the results of these observations the suggestion came that such influences might throw light on the difference be-

tween sensory and motor reaction-time. The following table shows a series of reactions before and after sorting cards as previously explained, given in thousandths of a second and obtained with a Hipp chronoscope:

Sensory.		Motor.	
Before Cards.	After Cards.	Before Cards.	After Cards.
128	161	117	124
131	123	103	131
215	134	116	121
131	146	111	116
130	151	116	117
197	151	119	115
165	148	123	117
147	114	104	107
181	129	118	110
141	140	115	109
126	161	126	103
157	143	142	105
129	107	158	118
116	123	122	108
133	137	130	109
136	115	140	115
154	131	136	121
138	110	232	104
134	117	149	114
214	126	108	129
139	164	117	107
121	191	115	111
151	132	130	146
130	136	131	98
171	140	156	119
160	129	121	116
155	200	123	126
140	166	107	113
215	135	126	113
180	121		
		Aver. 128.3	Aver. 118.6
Aver. 152.1	Aver. 139.3	Av. var. 15.5	Av. var. 8.06
Av. var. 19.1	Av. var. 16.6		

These differences in reaction-times—which are relative and not absolute—must depend chiefly and primarily on changed central conditions; for even if, as has been suggested, the muscles are in a greater state of tension after such mental exercises, it is a result of an unconscious partial innervation of the muscles of the body accompanying increased central activity. If it be true that general muscular tension is produced by mental concentration, it leads to the very important suggestion, viz.: that the chief cause of the feeling of bodily

weariness resulting from mental concentration, may be due to muscular fatigue.

Experiments on Others. Experiments were made at my request by two other men. They followed the same programme that I had used. Records were taken for seventeen days by B., and for six days by L. In neither case was there developed any marked daily rhythm. B.'s averages for the different hours, show the greatest rapidity at 8 a. m. and the least at 6 p. m., while the rate at the other four hours was approximately the same. The mean variation is so large, however, that these averages are comparatively meaningless. The daily rhythm in my own case did not make its characteristic appearance until after almost a month's practice. The only definite results from the work of B. and L. are their rates and the effect of exercise which will be spoken of further on. B.'s rate for the whole series of tests made was 6.4 taps per second; for L. the rate was 6.8 taps per second.

During the course of the experiments tests were made upon a number of visitors. The accompanying table shows their rates, and is of interest in that great individual differences are shown.

RATE OF VISITORS.

Date.	Individual.	Time of Day.	Time in sec. for 300 taps.		Date.	Individual.	Time of Day.	Time in sec. for 300 taps.	
			Right Hand.	Left Hand.				Right Hand.	Left Hand.
1892					1892				
Feb. 5	A.	11.10 a. m.	45	68	Mar. 8	L.	2.30 p. m.	47	
" "	B.	11.30 a. m.	46	49	" 12	M.	5.30 p. m.	57	
" "	C.	11.00 a. m.	42.5	51.5	" 12	N.	5.30 p. m.	47.25	
" "	D.	6.00 p. m.	46	50	Apr. 4	O.	10.00 a. m.	53	
" "	E.	9.00 a. m.	44	57	" 10	P.	12.00 m.	50	
" 9	F.	10.30 a. m.	45	60	" 10	Q.	1.00 p. m.	67	
" 9	G.	4.00 p. m.	46	54	" 10	R.	1.00 p. m.	51.5	62
" 9	H.	4.10 p. m.	49		" 15	S.	2.00 p. m.	66	
" 9	I.	4.15 p. m.	51.5		" 15	T.	2.00 p. m.	57	
" 22	J.	4.30 p. m.	52						
Mar. 4	K.	4.00 p. m.	48						

Effect of Practice. The average rates of B. and L. were reached on the third day, and no perceptible increase came in the remainder of two weeks' work. The increased normal rate seemed to be chiefly due to the fact that fatigue ceased, which was shown in the greater regularity of the curves. All visitors who took a record for the first time complained of fatigue, and the change in the direction of their curve toward the close of a record plainly showed their diminishing rate. In my own case the average ability had been attained while testing and perfecting the apparatus previous to recording any work. In order to test the effect of exercise of the right hand upon the left, the rate of the left was found at the beginning of the experiment, and again, after more than two months of work. The result shows no gain in the left. If the rate of the left had been influenced any it occurred before any records were taken.

Effect of Rest. The effect of a day of rest, when the subject was in good health, was very slight as shown by comparing the records of two Saturdays and two following Mondays:

Saturdays.							Mondays.						
Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.	Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.
Time in sec. for 300 taps.	43	41	36	40	36	40	Time in sec. for 300 taps.	42	43	38.5	43	40	43.25
	39	37.5	36.5	40	36	38		40.5	40	39	40	36	42

From the above table it is seen that the control was slightly weakened for Mondays rather than strengthened; a marked difference is shown between two Saturdays and the following Mondays, during a period of overwork, resulting in a week's illness:

Saturdays.							Mondays.						
Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.	Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.
Time in sec. for 300 taps.	37	38.5	37.5	39.5	34.5	37	Time in sec. for 300 taps.	38.75	32.5	32.6	30	30.5	32
	37	35	35	36.5	34.5	Ill		40.5	36	38.5	33	Ill	Ill

These observations are recorded merely from their suggestiveness.

Although we have subjective evidence of varying degrees of mental activity we have very few methods of accurately estimating its condition. The fact that central activity is accompanied with the power to make rapid voluntary movements suggests that the rate at which voluntary movements can be made, may be taken as something of an index to central nervous activity. The following facts may be stated as the chief results of this research:

1. The apparatus described furnishes a convenient and accurate method of recording and counting rapid voluntary movements.

2. The normal rate for the most rapid voluntary movements of the right wrist was found to average 8.5 taps per second.

3. During the work a daily rhythm was developed with the slowest rate at 8 a. m., and the most rapid at 4 p. m.

4. A vigorous walk decreases the rate for rapid voluntary movements of the wrist.

5. Increased central activity favors increased rapidity for voluntary movements.

EXPERIMENTAL RESEARCH UPON THE PHENOMENA OF ATTENTION.

By James R. Angell and Arthur H. Pierce.

These experiments have been carried on in the Harvard Laboratory with the purpose of discovering the correct interpretation of some curious results first obtained by Prof. Wundt in the study of attention. Prof. James and Prof. Wundt differ in their explanation of the facts and we have attempted to ascertain which, if either, was right. We have come to a conclusion differing from both, which we venture to offer with some assurance that our results will bear the test of verification by other observers. At the outset we shall state briefly the conditions of the experiments as conducted by Wundt and the explanations adduced by him and by Prof. James.

The essential question is whether we can interpret *as simultaneous* two or more disparate simultaneous sensations, and if not, how to explain our errors. For example, as Wundt conducted the experiment, the observer is seated before a graduated dial over which moves a pointer like the minute hand of a clock. The hand does not make continuous revolutions, but vibrates back and forth, each vibration traversing a trifle less than a circumference. A mechanical device enables a bell to be rung at adjustable positions of the pointer, and the subject is required to fix the location of the hand when the sound is heard. The experiment can be complicated, as was done by von Tschisch, through adding electric and tactile stimuli, but his results from these variations show no essentially new factors and are easily explicable under our hypothesis. Furthermore the fundamental conditions are all present in the simpler form, so we shall hereafter deal with that alone. Now, Wundt, found three classes of results. First, *correct results*, when the sound was heard at the position of the pointer where it actually occurred. Second, *positive displacements*, when the sound was heard at a position of the pointer after it had passed the real place. Third, *negative displacements*, when the sound was heard before the pointer had come to the real place. When the

revolutions occurred once in a second he found the errors least frequent. When the rate was faster positive errors predominated, when slower, negative. Wundt has very properly emphasized the peculiarities of different individuals and the differences in the same individual at different times. In this we heartily agree with him and it is no part of our purpose to question any results he has attained. But his explanation strikes us as faulty. He would account for all the variations by the peculiar laws of the ripening of apperception. Thus he assumes that the apperception keeps periodically ripening after each stroke of the bell in anticipation of the next stroke. It may ripen more slowly or more rapidly than the occurrence of the strokes. If faster, then the sound is heard too soon. If slower, it is heard too late. In any event the position of the index at which it is heard is identified as the correct one, and this is obviously determined by the rate of ripening peculiar to the individual. A word should be said at this point in explanation of Wundt's actual procedure in an experiment. He always finds a single revolution of the pointer insufficient to locate the position at which the sound occurs. The motion must continue for a sufficient length of time to allow the sounds to form a regular series. A certain region of the dial is then perceived as the general location, and individual points in this region are then selected until the mind is satisfied.[1]

The general nature of Prof. James's criticism will appear from the following quotation from his comments on this subject (Prin. Psych. page 415, Vol. I.): "The bell or other signal gives a momentary sensation, the index a continuous one, of motion. To note any one *position* of the latter is to interrupt this sensation of motion and substitute an entirely different percept—one, namely, of position—for it during a time however brief. This involves a sudden change in the manner of attending to the revolutions of the index; which change *ought* to take place neither sooner nor later than the momentary impression, and *fix* the index as it is then and there visible. Now this is not a case of simply getting two sensations at once and so feeling them—which would be an harmonious act; but of *stopping one* and changing it into another (i. e. exchanging one for another) whilst we simultaneously get a third. Two of these acts are discrepant, and the whole three rather interfere with each other. It becomes hard to 'fix' the index at the very instant that we

[1] For a fuller account of the matter see Physiol. Psych. 2nd ed. II. 264-6, 273-4; 3rd ed. II. 339; Philos. Studien, II. 601 ff. Also Prof. James's Psychol. I. 410 ff.

catch the momentary impression; so we fall into a way of fixing it either at the last possible moment before, or at the first possible moment after, the impression comes. This at least seems to me the more probable state of affairs. If we fix the index before the impression really comes, that means that we perceive it too late. But why do we fix it *before* when the impressions come slow and simple, and *after* when they come rapid and complex? And why under certain conditions is there no displacement at all? The answer which suggests itself is that when there is just enough leisure between the impressions for the attention to adapt itself comfortably both to them and the index (one second in Wundt's experiments), it carries on the two processes at once; when the leisure is excessive, the attention, following its own laws of ripening, and being *ready* to note the index before the other impression comes, notes it *then*, since that is the moment of easiest action, whilst the impression, which comes a moment later, interferes with noting it again; and finally, that when the leisure is insufficient, the momentary impressions, being the more fixed data, are attended to first and the index is fixed a little later on. The noting of the index at too early a moment would be the noting of a real fact, with its analogue in many other rhythmical experiences. . . . . Wundt's explanation (if I understand it) of the experiments requires us to believe that an observer . . . . shall steadily and without exception get an hallucination of a bell-stroke before the latter occurs, and *not hear the real bell-stroke afterwards*. I doubt whether this is possible, and I can think of no analogue to it in the rest of our experience."

It is no part of our purpose to follow out the differences of opinion merely as such which exist between Professors Wundt and James concerning this matter. We trust the above statement has made their respective positions clear while bringing out the exact nature of the experiment and the points at issue. We believe that there is truth in the explanations of both, but our criticism is that neither at all suffices to exhaust the problem, which proves to be much more complicated than one could possibly suppose from the mere reading of an account. Wundt has justly remarked that the research requires years of experimentation before adequate conclusions can be reached. And yet this is not wholly true. Wundt has worked many years on the subject himself, but his main interest has apparently been to obtain a sufficiently large number of results to justify comprehensive and exhaustive inductions. So far so good. We are, however, unable to see that careful experimentation covering a much shorter period is necessarily imcompetent to isolate with considerable

accuracy the essential psychological processes involved. We think Herr Wundt has overlooked some very important factors and it is on this account that we venture to offer our conclusions. By no means would we on any other ground oppose our experiments, which are only numbered by hundreds, to his, which must mount into thousands.

In front of the dial of our apparatus revolved a pointer, carrying at the back a wire, bent so as to sweep through a movable cup of mercury at the rear of the dial, thus completing an electric circuit which passed through a telegraph sounder, the latter giving a loud click whenever the circuit was closed.

Various reasons led us to abandon Wundt's machine. The specimen at our disposal was faultily constructed in several particulars and accurate experimentation with it seemed almost impossible. In addition to this, the noise made by the clock work, which was the motor power, was very distracting to the observer and we felt sure it must vitiate the results. The principle of the pendulum seems to us unfortunate in this experiment, because the pointer starts from perfect quiet, moves rapidly faster and then slows up to quiet again, thus introducing a constantly changing rate of motion. Most objectionable of all was the bell. The vibrations of course last some time, and the sound of the actual stroke becomes so blended with this after-tone that the difficulties of identifying the stroke with any one position of the pointer are vastly increased. An attempt was therefore made to construct a machine which should possess the following qualifications: first, lack of distracting noises; second, uniformity and steadiness of motion in the rotating pointer; third, an incoming sound which should be short and flat.

The motor power which appeared most convenient and reliable was an ordinary kymograph. To be sure, this is not a noiseless instrument, owing to the whir of the governor, but the noise is by no means of the nature of a disturbing click and we have had no complaint of disturbance from this source on the part of any of our subjects. Of course the ideal motor would be noiseless. The motion was transmitted from the drum of the kymograph to the pointer-shaft by means of a belt of ¾-inch webbing. As an upright to support the pointer a Bradley color-wheel was utilized, the pointer being set on the shaft in the place usually occupied by the color disc. The pointer, made of thin, light wood, gouged on the back to reduce the weight as much as possible, was 63 cm. in length, the tapering end having a length of 47 cm. from the center of the shaft. Pieces of sheet-copper were fastened to the upper end to secure an accurate balance and tests were made occasionally during the observations to ascertain if the equilibrium was perfect.

A dial was made of heavy pasteboard held upright by small wooden supports at each end, which were sunk into a strip of wood; as shown in

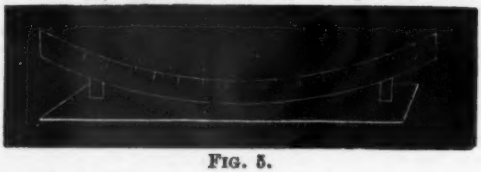


FIG. 5.

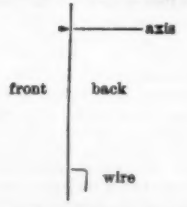
the accompanying figure. The graduation of the dial and the numbering should be as uniform as possible in order that no one portion may

present any greater interest than another. The slightest thing is sufficient to divert attention and so vitiate results.

As the figure shows, the dial was only a portion of a circumference. The inner edge of the dial was 43 cm. from the center, so the *entire* inner circumference would have been 270.18 cm. in length. The available portion of the scale was 47 cm. long—a little more than $\frac{1}{6}$ the entire circumference. This, however, was deemed long enough, as it gave considerable range to vision without requiring any rotation of the head and much less of the eye-balls than would have been necessary with a larger portion of the circumference.

To secure the short, flat sound so desirable we employed an electric telegraph sounder. At the back of the dial and separated from it by the width of its wooden supports was fastened a brass rod of about 3 mm. diameter and of the same curvature as the dial itself. On this ran a carriage bearing a cup of mercury. Down the back of the pointer from the shaft on which it rotated ran a wire which, sweeping through the mercury, completed the circuit passing through the key. The carriage was constructed as follows: upon a small wooden block through which the curved brass rod passed was fastened a small wire-coupler or binding screw. To the head of the screw a bit of glass tubing 6 mm. bore was secured by means of sealing wax. This tube contained the mercury. A small brass wire from the metallic part of the carriage was wound several times around the rod on which the carriage traveled, thus making an electric connection between the rod and mercury. The tension of the wood upon the rod was sufficient to hold the carriage steadily in whatever position it was put, and at the same time not enough to prevent an easy movement from place to place. The whole of the metallic portion of the carriage could be rotated slightly in a plane parallel to the dial and the mercury cup could be raised or lowered by the screw on which it was fastened, thus rendering any necessary adjustment easy and rapid. The carriage, with all that pertained to it, was wholly concealed from the view of the observer, whose eyes were on a level with the dial.

The light brass wire running down the back of the pointer was bent near the top of the latter, as the accompanying diagram shows.



So that while the pointer revolved in front of the dial-scale the end of the wire swept through the meniscus of the mercury behind the dial. A Leclanché battery gave entire satisfaction throughout. The circuit was as follows: From one pole of the battery through the curved brass rod to the mercury; thence, when the pointer completed the circuit by sweeping the mercury, up the pointer to the metal shaft, on which rested a platinum wire connected with the key, and so back to the other pole of the battery. Whenever the circuit was closed the key gave a sharp click.

The dial was divided into spaces of about a centimeter in length, numbered from left to right. The outer gauge of the kymograph and the internal changes of gearing made a large number of rates possible, but

we selected certain ones for the sake of uniformity to represent slow, medium and rapid movement. The time of rotation at these various rates was carefully ascertained by means of a metronome with the following results:

Slow rate—One revolution in 6.06 seconds. Outer gauge at 15. Lower wheel of kymograph in gear, with upper movable wheel in its middle position.

Medium rate—One revolution in 3.6 seconds. Same gearing as above with gauge at 30.

Fast rate—One revolution in 1.2 seconds. Lower wheel out of gear, and upper wheel at extreme left, i. e., toward inner wall of case. Gauge at 30.

The time occupied by the pointer in crossing a single space was accordingly .0219 sec., .0152 sec., .0043 sec., respectively for the different rates.

With these preliminary considerations we are now ready to consider our method of procedure and results. We may say at once that we meet the same facts as Wundt, i. e., correct results, positive errors and negative errors, though the ratio existing between the two sorts of errors does not correspond, nor yet the conditions under which they are found appearing. This, however, does not alter the fact that we have identical cases to explain. A word or two describing the modus operandi of a typical experiment will serve to clear up future explanation. Our usage has been this—to follow the pointer on its first revolution until the sound is heard, when we attempt to stop the movements of the eyes instantaneously. The point on the dial thus attained is made the basis of observation for the next revolution when any seemingly needed correction is made; to the right, if the sound seems to occur after the pointer has passed the given point; to the left, if before. But now in this very process of correction enters in a *double* process which neither of us observed at the outset, though it was later seen to play a great part. Fortunately it subjects itself with some readiness to independent experiment. What seems at first a bare attempt to get a coincidence of the sound with a certain position of the pointer proves on more acute observation to be a vibratory process in which attention is *primarily* directed in alteration now to the visual factor and now to the auditory. Thus our natural method seemed to be to pick out an approximate point on the dial and then watch for its obliteration by the revolving pointer. Immediately it was obliterated, the attention was turned to the sound to determine whether it had occurred or not. By repeating this process and making needed corrections, a point was attained on the dial which seemed to minimize the interval between the sound and the sight of the pointer obliterating the spot. We say

"minimize" advisedly, because we early discovered how easily one can be deceived into believing a point quite remote from the correct one to be the right one. We could not discover that there was any greater feeling of surety when a correct result was obtained than when an erroneous one. The two sensations *seem* to be essentially simultaneous and yet there is seldom any feeling of security. But in the method just described the obvious attempt was to make the visual element fundamental and then to hitch on the auditory. We were trying to attend to the sound *after* we had gotten a certain sensation of sight. Further introspection showed that however genuine was our attempt thus to exalt the visual factor, the truth really was that attention vibrated and sometimes the sight, sometimes the sound, was made fundamental. If we may be allowed a homely metaphor, now one element, now the other, served as hitching post. Before going on to discuss the varying results we may mention a fact which seemed to modify the above method in a measure. One of us noticed a strong sense of rhythm from the sounds. This was observed expressing itself in more or less unconscious muscular contractions and movements, such as the nodding of the head, the beating of the fingers, etc. This naturally suggests Wundt's ripening of apperception. What part this plays in the results will be remarked later on. Though we lay no stress on this portion of our work, it may be fair to state that the readings which we have obtained from first revolutions show almost without exception positive errors. The first twenty-five final readings of each new subject generally show a predominance of negative errors and then the positive errors come rapidly to the front. So that differing from Wundt we find a preponderance of positive errors. Furthermore we have been unable to detect any constant influence upon the character of the errors due to alterations of speed. We have made this test on the Wundt machine as well as our own, using the extremes of speed.

Now let us turn our attention to the explanation of positive errors in which the sound is identified with a position of the pointer beyond the place where the sound has occurred. This is the error which common sense would assume as natural. In the cases where we obtain a reading from the first revolution of the pointer, the positive error is easily explained as a dragging of the eyes by the moving pointer. It amounts here to a simple reactive experiment. The positive error is explained by the time consumed in transforming the incoming auditory sensation into a motor act, which is in this case the stopping of the eyes. When the eyes have actually stopped, time has therefore elapsed and the pointer having passed on

is thus read too far to the right. The positive error made after deliberation and also after long practice requires further explanation. If we represent the auditory and visual elements by their initial letters, we may express the vibrations which we find occurring in this way—either V—A—V or simply A—V. In the first case attention is directed to the dial, watching for the obliteration of the selected spot by the pointer. Suddenly it is shifted to catch the sound, and this occurring at once, or a moment later, the attention is shifted back again to the pointer, which is now naturally a trifle beyond the actual point of connection. In the second case, A—V, the first step in the above process is omitted. Indeed, it may be disregarded in any event, as the last vibration is the one affecting the result and giving a positive error. When the great ease with which attention fatigues is remembered, the basis of this explanation will be seen to have great strength, and actual experiment will add to its power. To prove the matter conclusively we made a number of experiments in which the sound was purposely made the primary element, to which we then tried to attach the sight of a given position of the pointer, that is, a position selected by the experimenter as approximate. All the errors under these conditions were positive and many of them very large.[1] Such an arrangement makes the experiment essentially one of reaction, though an apperceptive reaction, and the positive error is amply provided for under the necessity of a certain lapse of time requisite for the sound to be registered in the brain and then identified as simultaneous with a given position of the pointer. It may be objected that this is an essential alteration of the real experiment. To this we in a manner assent, simply insisting that the experiment is of such nature as to assume of itself just this (among others) unexpected form. What we have done is by no means to arrange artificial variations, but simply to observe the metamorphoses undergone by the experiment itself. It was not without reason that we remarked at the outset the exceeding complexity of the problem. Occasional positive errors, like occasional

[1] Confirmatory of this are the results of von Tschisch, who found the errors becoming positive when other interrupting sensations—as tactile and electric—were added to sound. The number and disparate quality of the interrupting sensations possessed strong interest, and the form of vibration, represented as A—V, occurred only after the attention had dwelt for a time on the combined sensations here grouped under A. Furthermore, the greater inherent interest of this complex stimulation, as compared with the dial and pointer, would tend to fix the attention in anticipation of these combined sensations and so we should obtain, as von Tschisch did, a preponderance of results in which the A—V vibration occurs, thus producing positive errors.

negative errors, are doubtless to be explained as due to the arbitrary and inexplicable fascination of the attention by some one spot on the dial which the observer then tends strongly to regard as the correct one. These freaks of attention, based on the unaccountable interest felt in infinitesimal differences of objects, must be assumed as constant. The positive errors, then, attained from readings of first revolutions are explicable on the basis of time consumed in mere reaction. Those occurring in later readings are also due to the lapse of time involved in an apperceptive reaction. In short, attention vibrates, and this vibration may be of sufficient strength to overpower the factors at work in negative errors and so by the time consumed in the process produce positive errors.

We must next consider negative errors. It will be recalled that we mentioned the fact that in our experiments negative errors predominated during the first twenty-five results, and from that time on positive errors constituted a large majority. In one case the first twenty-five results were all negative errors, while in the ensuing hundred and twenty-five only three negative errors occurred. We may state at once that we consider the factor ordinarily most efficacious in producing negative results to be unconscious, or involuntary, over-correction, which we shall proceed to elucidate. Experience teaches mankind that in the case of moving objects, similar to the pointer, the attempt to stop the eyes from following has generally resulted in their being carried along a little beyond the point desired, and we have become accustomed to rectifying this mistake. What was once a conscious effort has now through psychic education become practically automatic. Instances of education of this general type are common enough. Learning to shoot on the wing, catching or batting a ball are excellent illustrations. But it is still competent for some one to object that this does not explain enough. It may explain why, after the eye has left the pointer, it should swing back to the left, but not why there should even then necessarily be a negative error. Why does not the eye stop at the right of the correct point and so give a positive error? To this we reply that the special rate of motion manifested by the pointer introduces an essentially new psychic, as well as physiological, factor. What we mean is that *rates* of motion as we experience them in life are so exceedingly various as to render accurate acquaintance with any of the faster forms, such as we deal with in this experiment, quite improbable. Thus the subject, when first experimented upon, is really encountering a unique set of experiences to which he has not yet learned to accommodate himself. We think that what happens physiologically (and so explaining

the negative error) is this: there occurs a too complete relaxation of the ocular muscles involved in making the eye follow the pointer, coupled with a spasmodic—and thus too severe—contraction of the reversing muscles. It is in truth an illustration of the difficulties encountered in the accommodation of an organism to a new environment. The case is perfectly analogous to the attempt to catch a falling goblet or vase, where one almost invariably spreads disaster through the spasmodic nature of his effort. Again it may be objected that while our explanation so far may hold good for the cases of first revolutions when the eyes are obviously following the pointer, it is considered fallacious as accounting for negative errors in later cases conducted as we have described, i. e., by watching for the obliteration of a given point by the pointer. The objection has only slight importance, because, no matter how intently the gaze be fixed on the dial, it is practically impossible to prevent a slight vibratory movement of the eyes when the pointer passes, and the spasmodic motion is only reduced in power, but by no means eliminated. In any event this is not the whole story about negative errors. We are, however, convinced that this unconscious over-correction plays a very large part in the production of these errors. In terms of our vibration formula this process would be expressed as V—A—V+ unconscious over-correction.

But the negative error may be produced in another way. Suppose, on a dial graduated and numbered like ours from 1 to 12, the space between 5 and 7 had been settled on by the observer as the general region in which the sound occurred. As the pointer comes round and gets to 5, the attention, which has just previously been concentrated on the visual factor, is instantly shifted to the auditory, and if that occurs in quick sequence the mind feels satisfied that the two have occurred simultaneously. Attention is in this case not shifted back to the visual element. The vibration is expressed as V—A. Apperception time doubtless plays a part here again, but while it enters in to cause error, it has of itself no especial preference for either kind of error so far as we know. A variation which we have next to notice may be wrongly charged to apperception, but we repeat, we know of no facts tending to show *with any conclusiveness* that the apperception time of sound is either longer or shorter than that of sight.

On the other hand there is experimental ground for believing that the photo-chemical process in the retina consumes an appreciably longer time than the vibratory process in the cochlea, so that sight is a slower affair than hearing. Ladd gives the ear an advantage of .049 sec. Now at the slowest

rate employed by us the pointer traversed one space in .0219 sec. Therefore the .049 sec., by which the eye is slower than the ear, is equivalent to more than two spaces traversed by the pointer. We are inclined to regard these particular figures as provisional and subject to alteration under more extended experimentation, but it may be accepted as proven that sight requires longer than hearing and enough longer to play no inconsiderable part in this experiment. Obviously the effect of this fact would be to cause negative errors, for, to take an example, when the pointer was *seen* at $6\frac{1}{2}$ (6 and two spaces) it would really be at 7, and if the correction was made at 7 the sound would be identified with the position $6\frac{1}{2}$ rather than 7, though the time consumed in transmitting the auditory sensation would throw the reading slightly forward toward 7. But it is clear that the effect of this condition of things must be overpowered occasionally by the processes mentioned in our discussion of positive errors, otherwise we should continually get negative results. Aside from our experiments, which point strongly to the fact, there is good *a priori* ground for supposing that experience teaches us to make correcting judgments to compensate for these discrepancies among our senses, so that the gradual disappearance of negative errors under practice is what we should expect, Wundt's results to the contrary notwithstanding. It should be understood that this transition to positive from negative errors occurs without the subject being informed of the nature of his errors, indeed without his knowing whether he is making any errors at all. It seems to be a result of finer powers of discrimination and attention resulting merely from practice. It seems highly probable, then, that when a subject has become practised in the experiment, the part played by unconscious correction of the oculo-motor effects and the tardiness of the photo-chemical processes gradually dwindles into insignificance, and negative errors, when they occur, which is rare with us, are rather due to the vibratory process of attention which we have indicated by V—A. Again we subjected our theory to separate experiment by consciously trying to make the visual factor primary and the auditory secondary without a return to the visual. The result substantiated our hypothesis. In a subject whose total results show a ratio of 3 : 1 in favor of positive errors, this procedure altered the ratio to 4 : 3 in favor of the negative errors. The experimentation was not sufficiently extended in this direction to warrant our ascribing any final validity to this test. It served to satisfy us quite conclusively, however, because the actuality of the process supposed showed itself clearly.

We mentioned earlier in the paper the observation of a feeling of rhythm gained from the succession of sounds and expressing itself in various muscular contractions. We have now to consider the effects of this. It will be remembered that Wundt's explanation hinges on the ripening of apperception. The *natural* rate of ripening may be faster, or slower than the rate of occurrence of the sounds, or identical with it. The different results found their explanation in this fact. Prof. James's explanation finds a somewhat similar basis. The rhythmic feeling we mention should have a very similar effect. With our machine, which gives its sound less frequently than Wundt's, we might naturally suppose that the impatience to "let off" the muscular contractions would produce a tendency to negative error. But there is nothing to substantiate any such sweeping assumption. The peculiarities of the individual may cause this to affect the result in either direction, and by most observers the feeling is not noticed at all. In short, it is another factor which we must consider constant in its influence.

What shall be said of the correct results? It would seem to be sufficient reason for assuming these to be accidental that one has no greater feeling of surety when giving a correct reading than when giving an erroneous one. It is by no means impossible that the different factors which enter into the experiment should occasionally so combine as to produce correct results. Similarly we see no reason for supposing that these same factors may not at any time so combine as to produce either kind of error. We admit the inherent charm of such accounts as those of Prof. Wundt and Prof. James, which seem to involve only a single general principle, but we cannot feel that these at all cover the ground. In any case we do not think the present kinds of apparatus at all competent to decide dogmatically whether the mind does ever succeed in detecting exact simultaneity in such cases aside from mere chance. Our machine is much more accurate in this respect than Wundt's, but it is exceedingly difficult to be sure that a given position of the index is *absolutely* and *exactly* simultaneous with the click. For our own part, we do not believe the mind ever does feel two such stimuli as exactly simultaneous in any other sense than that in which it feels rapily successive and disparate stimuli to be simultaneous. Excessively rapid vibration it is capable of and this answers its purpose. But apparently it has no criterion by which to distinguish exact similarity of disparate stimulations from rapid sequence of the same.

But now a word about Wundt's method of conducting the task which he set himself. The problem which he proposed

he states thus: to examine what shall happen "when we receive a *series* of impressions *separated by a distinct interval* into the midst of which a heterogeneous impression is suddenly brought." But in the method adopted does Wundt really accomplish his avowed purpose? Does the moving of an index hand over a scale really give a *series of impressions separated by a distinct interval?* Not only is this not strictly speaking the case even if one succeeds in following the index hand round the dial, but most of all it is not the case when one adopts the method of fixating a point on the scale for trial reference, repeating the process until satisfaction is gained. Wundt apparently followed the former method, but even if the attempt is thus made to follow the pointer constantly, one soon discovers that he is dealing with no series of impressions separated by distinct intervals. Some portions of the scale will be seen with much greater clearness than others. The constantly changing rate of motion in the hand of Wundt's machine, owing to the pendulum principle employed, adds greatly to this defect inherent in the observation of any body moving across a graduated scale. To obviate this difficulty we attempted to construct an apparatus by means of which a *genuine series of impressions separated by distinct intervals* should be attained. We succeeded in producing a device by which the letters of a series were presented to the eye one at a time. The sound was made to coincide with one of the letters. Two forms of the apparatus were employed, the differences between them appearing from the following description:

FIRST FORM.—Nearly the same materials were used as in the machine previously described. The scale and pointer were removed to the rear of the support, the pointer being attached to the other end of the shaft. Both scale and pointer served simply to maintain the old method of electric connection. At the front, in the place formerly occupied by the pointer, was placed a circular piece of paste-board, 40.5 cm. in diameter, at the circumference of which were placed the letters of the alphabet arranged at regular intervals. The letters were from a set of Dennison's alphabets, size 31. The circle bearing the letters was made to revolve as before by means of the kymograph. About six inches in front of the revolving circle was placed a wooden box large enough to admit the head with ease and not too deep to offer inconvenience to the observer. In the back of this was cut a window 1 cm. square and exactly on a level with the position of the lowest letter on the circle. The box was blackened on the inside to prevent any distraction to the eyes. The position of the box and its distance from the circle could of course be adjusted to the observer. The circle being fitted on to the shaft like a color-disc, it was only necessary to rotate it on its axis to secure the exposure of any letter and then fasten
ening the screw which held it in position.

SECOND FORM.—In the form just described the observations were necessarily made with the use of only one eye. The speedy fatigue under these conditions led us to alter the form as follows: In place of the

revolving circle bearing letters to be viewed through a stationary window, we utilized a circle of black paste-board slightly larger than the last and like it revolving on the shaft. Near its edge a small window was cut through which a series of stationary letters behind could be viewed as the window exposed them one by one. We used a smaller set of letters for this, No. 21. They were arranged upon the arc of a circle whose radius was equal to the distance from the center of the shaft to the lower edge of the window in the paste-board circle. They were separated from each other by spaces of about 1 cm. The adjustments could be made as before. To avoid excessive rotation of the head and eyes, we arranged so that only about $\frac{1}{4}$ of a circumference was exposed for experiment and this on a level with the observer's eyes.

These variations in form have had two points of value for our conclusions. In the first place we find that when you have a genuine series of visual factors regularly succeeding one another your results become disturbed by the effects of after-images. The consequence is an increase in negative errors. For example, suppose in our experiments the electric contact was made at the letter C. It frequently happened that the after image of B or A (which had gone before) would come out with sufficient vividness to be identified with the sound and the attention, being then *set* for A or B on the next revolution, would tend to settle on one of them as the correct letter. The occasional vividness of these after images was very marked and we can now see that it may well play some part in the original form of the experiment. The second consideration is this: that conducting the experiment as Wundt proposes with a series of impressions separated by regular intervals into which is introduced a heterogeneous impression, there is no appreciable effect upon the result other than the effect just noticed of allowing after images to play conspicuous part. This is an additional reason in our minds for distrusting any explanation which regards the facts as capable of arrangement under any one general law, such as the ripening of apperception. The factors causing the different results are far too heterogeneous to admit any such arrangement and, so far as we can see, they *may* at *any time* combine in such relation as to produce either kind of result.

In conclusion, allow us to repeat that we by no means regard our results as warranting any very comprehensive inductions. We question the possibility of making such for many years to come. We hope to have done some service in bringing out a few of the peculiarities in the phenomena of attention and to have suggested in a measure the unexhausted richness of a very simple experiment in revealing these. We take great pleasure in acknowledging our obligations to Prof. James and Dr. Herbert Nichols for much valuable advice and suggestion.

SOME EFFECTS OF CONTRAST.

By A. Kirschmann, Ph. D., of Leipzig.

1. *Kinds of Simultaneous Contrast.* The forms of simultaneous contrast in the domain of sight can be reduced to the following:

1. Contrast in brightness.
2. Contrast in saturation.
3. Contrast in color.
4. Contrast in emotional tone.

The contrast in the size of the surface is omitted as belonging to all the space senses.

These kinds of contrast depend on certain conditions. The strength of the brightness-, color- and saturation-contrasts depends on the intensity of the light emitted from the object; the strength of the color- and saturation-contrasts is dependent on the shade of color and the degree of saturation of the contrasted surfaces; the strength of the contrast is also dependent on the extent of the contrasting surfaces and on the distance of the objects from one another.

The relations of the various kinds of contrast to these variables I have investigated. The results which have been published elsewhere[1] can be summarized as follows:

The intensity of pure simultaneous brightness-contrast, and probably also of the pure simultaneous color-contrast, increases, within the limits of clear perception of size in the resting eye, proportional to the linear extent of the inducing part of the retina, or proportional to the square root of the surface content. An intensity that produces a contrast can be replaced by a less intensity of a correspondingly larger extent without changing the strength of the contrast; that is to say, a relation of reciprocity obtains between the extent and the intensity of the colors that enter into the contrast. The simultaneous color contrast appears best when the brightness-contrast is excluded or reduced to a minimum. The simultaneous contrast between a color impression and a gray of

[1] Kirschmann. *Ueber die quantitativen Verhältnisse des simultanen Helligkeits- und Farben-Contrastes*, Inaug.-Diss., Leipzig, 1890; also in Wundt's Philos. Studien, 1891 VI. 417.

equal brightness increases with the saturation of the inducing color. The simultaneous contrast between two colors is composed of two factors, the quantitative relations of which with a regular increase or decrease of the saturation of the colors change in irregular manner and in opposite directions. The mutual contrast between two colors reaches its maximum with a combination of medium degrees of saturation of the colors.

2. *Black and White.* Among the effects of contrast there is one to which attention has not been called, although a neglect of the influence of contrast has led to a fatal error in Hering's theory of the fundamental colors. As is well known, his theory asserts the production of the various colors by the greater or less preponderance of the associative or the dissociative processes between two opposed pairs of fundamental colors. Likewise black and white are two such fundamental sensations and our different sensations of brightness without color are produced according to the preponderance of the one or the other of the processes in this pair. This assertation that black and white are elementary sensations is unjustified, as I shall attempt to show.

Right here, however, I must call attention to the fact that I do not have anything to say against the sensations of brightness, or the achromatic series of sensations of light, as such; this is, of course, too amply proved to need mention. But I do object to the unproved assumption that this series is produced by two antagonistic sensations just as a simple color is produced. On the other hand I am quite ready to agree to the view that black and white are extremes of a series which is not analogous to a series of simple colors, but to the degrees of saturation of a color; that is, white and black are not the extremes in the same way as red and blue-green are the extremes of the series of colors from red to blue-green, but are extremes in the same way that colorless light and the highest degree of saturation are the extremes of a series of any color in regard to its saturation.

Hering's theory regards colors as lying in two bipolar systems which have a point of indifference. If, e. g., the sensation red corresponds to the condition of assimilation and the sensation of blue-green to the condition of dissimilation of that particular substance, then colorlessness denotes the condition where the activity of the substance is zero. If we apply these relations to the black-white series, then the point of indifference is to be sought in the middle gray; to regard the middle gray as the zero-point of the activity of the black-white substance is a hypothesis that is not to be accepted lightly.

Are white and black really sensations? Hering's theory requires an affirmative answer to this; nevertheless the facts require, as I intend to show, a negative one. A phenomenon is to be considered a sensation, i. e., a simple mental phenomenon, when it cannot be further analyzed. It must maintain its character qualitatively unchanged even when isolated from all other mental elements. For example, the sensation red is not necessarily bound to any other phenomenon of consciousness. We always recognize this particular quality of light as red, no matter what the surrounding conditions may be, provided there is nothing abnormal about our organs of sight. In order to recognize an object as red

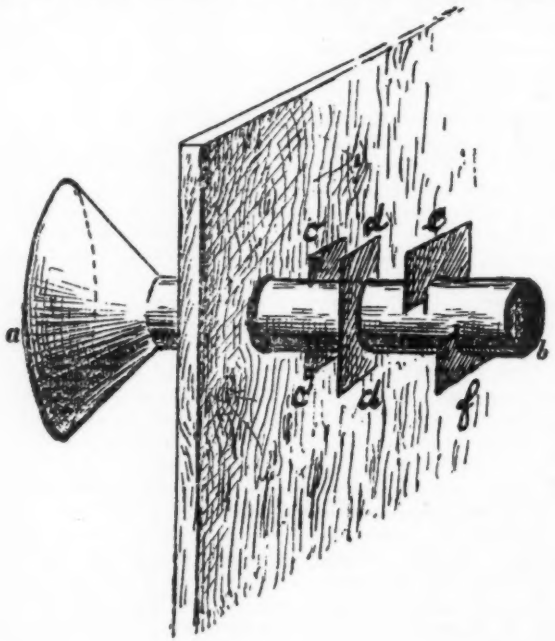


FIG. 6.

nothing more is necessary than that we see it; isolate it from surrounding objects or bring it into any relation to them, it never appears blue or violet, but always red. Surrounding objects, however, do have a great influence on the degree of saturation; the shade of a given red will be different accord-

ing to various influences. Can we say the same of black and white as of red and the other colors? I will first describe some experiments that answer this question, after which a number of familiar illustrations will be given.

The apparatus, which is not difficult of construction, is shown in figure 6. A wide tube *a b* is painted outside and inside with Paris black and is inserted through the door of the dark room so that the wider end is outside while *b* is in perfect darkness. Four diaphragms, *c*, *d*, *e* and *f*, are placed in corresponding slits in the tube in such a way that an eye looking through the tube sees an opening about two cm. square. They are placed at a distance from one another so that even the small quantity of light reflected from the walls of the tube is kept back from the neighborhood of the square opening. When in the dark room various colored or colorless surfaces are brought before the end of the tube, the observer at *a* is able to recognize only the intensity and the quality of the light reflected from the surface as long as the surface is so held that the edge of it cannot be seen within the square. By changing the illumination (lamp-light, gas-light, daylight), by changing the position of the reflecting surface to the direction of incidence of the illuminating rays and finally by the interposition of colored and gray glasses, the quality and intensity of the reflected light can be changed at will without the observer knowing anything more than the actual change on the surface observed. The observer was called upon to name the various lights and colors seen through the opening. A series of so-called blacks, e. g. black cardboards, dull black paper, Paris black, etc., was first presented before the tube so that nothing more than the surface was seen. Dr. Leitzmann, who was kind enough to serve as observer in these experiments, was called upon to name the colors. He did not designate a single one as black, but every one as gray, dark gray, a trifle light, somewhat light, rather light, etc. The term black was used only in the case where the opening was completely closed by the diaphragms. Even the best Paris black and the still blacker shadow that can be cast on it were perceived as light; indeed, by very strong illumination they produced the impression of light gray, and on one occasion, by a sudden change from daylight to gas-light, that of yellow. White papers and cardboards were not recognized as white, but, according to the illumination, as light, light gray, very light, light yellow, etc. With great decrease of the illumination they were gray, dark gray, very dark. Only once was an impression recognized as white, and this was from a piece of dirty gray but strongly illuminated wrapping paper. On the other hand colored papers were always correctly

recognized, provided the saturation was sufficient for the contrast with the black field of vision not to render the color entirely unnoticeable.

This fact, that our blacks and whites are in reality only grays, is seen in many every-day experiences. That we name an object black or white rather than gray does not depend alone on the quality and the intensity of the sensation of light, but essentially on the fulfillment of other conditions that have nothing to do with the sense of sight. Here we find not only the various properties of the object itself, but also two entirely subjective factors. The first of these is the peculiarity of the sense of sight mentioned in a previous treatise,[1] by means of which on the foundation of the intensities in the field of vision we form for ourselves a maximum of brightness that is not to be exceeded in our reproduced ideas until brightnesses of greater strength enter the field of vision. On a dull autumn day let the eye be fixed on a sheet of white paper and try to imagine fresh fallen snow in a degree of brightness exceeding that of the paper; it will be difficult or impossible, because the memory of the snow is too weak to compete with perception. The case is different when in winter we come directly from the snow-covered street into the room and make the experiment, because what is said of the brightnesses in the field of vision of course holds good also for the very lively memories of strong impressions which have just disappeared. Exactly the same can be shown of the saturation of the colors. Upon looking at a sheet of the best quality of red Heidelberg paper we cannot succeed in imagining a still finer, more saturated red, although the pigment does not produce the highest degree of possible purity and saturation. Of this we can easily convince ourselves, if we compare it with spectral red or even only with red glass by transmitted light or with the best silk velvet. This relativity of the maxima of brightness and saturation—which is to be regarded as a necessary result of the general law of relation—is only one reason why we are inclined to call brightest and most colorless objects in the field of vision by the name "white" and the darkest and most colorless objects by the name "black."

The second of the above mentioned factors is the ever-ruling inclination to correct the present sense-perceptions by connection with previous contents of consciousness, i. e. out of the present impressions and the memories of previous impressions called forth by the given object or those like it we form a complex of sensations or percepts, which necessitates

[1] KIRSCHMANN, *Ueber die Helligkeitsempfindung im indirecten Sehen*, Philos. Studien, 1889 V. 481.

us, on account of the constancy needful for a later recognition, to abstract from some of the components or at least to give them a less degree of attention. We recognize the domestic cat in every illumination, although the visual presentation can differ to an extreme degree in regard to intensity. We pay no essential attention to the illumination, although the presentation was only possible through it. In other words, we represent to ourselves the things not as we see them at the moment, but as we know that they are. When we ask a four to six year old child to draw a face on the slate, it very often happens that a profile with two eyes is produced. The explanation seems to show the same tendency as that just mentioned. The child has often enough seen human beings in profile, but the eye is the most important part of the face and he cannot represent a human being to himself without the two eyes, and he thus corrects his presentation of faces in profile by the adding the other eye. This is exactly the same as the case of every student of drawing. Errors in perspective are constantly made because the person draws things as they are and not as he sees them. An amateur, for example, in painting a winter landscape will hesitate to picture the deep shadows on the snow and he will draw the snow perfectly white, as he thinks it to be.

Black, according to Wundt, is the sensation from the complete inactivity of the retina, but it never occurs in one with normal eyes, since special retinal light is never absent. In external objects the nearest possible approach to black is an extremely dark blue. The best Chinese ink, which appears as the deepest black when drawn as a line on white paper, is readily seen to be a gray when it is used to cover a large surface. Even Paris black, which is the blackest of all pigment, is not a full black, for on a surface covered with this black we can draw still blacker lines with the same pigment. Compared with the inner surface of a black cylinder, the Paris black appears distinctly gray. It is just the same with writing-ink or printer's ink; it appears black to us only so long as it is in small lines or letters or numbers on a bright background, where the simultaneous and the border contrasts are so weakened that the difference between the characters in pigment and the retinal light is not perceived. In short, every case where we believe ourselves to see complete absence of light is due to contrast in brightness.

We designate those surfaces as white, concerning which we have learned by experience that with every illumination they possess the maximum capability of reflection and colorlessness. The swan is white, even when it is in the deepest shadow. Even the shadow on the feathers of the swan no one would

call gray, although they are far darker than the lights. A piece of white paper can be held at such an angle to the incident light that it is really not so light as a piece of gray paper beside it, which has a better illumination, and no one would call the former gray; yet we believe that we see the former as white and the latter as gray. This, however, happens only as long as we know what the shades of the two really are. If we do not know this, then we judge the two as we actually see them, just as in the experiment described above.

From these considerations it seems to me clear that in order to attribute to an object the property "black" or "white," we need to know more than the quality and intensity of the illumination of the objects and that these designations are not designations of sensations, but of judgments. Black and white are thus concepts and not sensations. There are white objects, but no sensation white, what we call white being simply a gray, with a surface that possesses the maximum degree of capability of reflection under the various conditions of illumination.

3. *Effect of Colored Illumination.* It is often said that in the evening by lamp-light the yellow objects appear almost white and are scarcely to be distinguished from white. This, however, is an illusion of the judgment; just the reverse is true. In reality the white objects appear yellow and the yellow objects still more yellow in the light of the lamp which is rich in red and yellow rays, but lacking in a corresponding quantity of blue rays. This is clearly seen when a piece of white cardboard is so arranged that half of it is illuminated by daylight and the other half by lamp-light, with the appropriate protection from extraneous light. The half lighted by the lamp appears decidedly yellow or pale orange, and yet in the evening under the same illumination, when we have nothing really white with which to compare it, we call it white.

Just the same thing happens in looking through weakly colored glasses or sheets of gelatine. We believe ourselves to see the objects in their proper colors, as though we could see one color through another. This supposition is incorrect, as is seen by looking at colored surfaces through the apparatus mentioned above in such a manner that neither the edges of the objects nor the character of their surfaces is seen. The mixed color produced by the color of the object and that of the glasses will then be seen and the illusion will disappear. The knowledge of their true colors which we have from general experience has led us to believe that we see the objects in their usual colors in spite of the influence of the glasses.

This explanation, however, is sufficient only in the case of slightly colored glasses. With glasses of more saturated colors there are also two additional factors that show themselves, the fatigue of the retina and the purely psychological influence which assists the recognition of the colors of the objects and an abstraction from the color of the glasses.

With a pair of spectacles, specially made by Krille in Leipzig, in which various colored glasses could be set and which were provided with side screens to keep out extraneous light, I was able to perform in a convenient way all the experiments described by Fechner in his essay on contrast.[1] My results do not differ essentially from his. When the spectacles are provided with blue, violet, yellow or green glasses, after a short time the impression of color disappears and the objects appear in their natural colors with the exception of those objects whose colors are extinguished by the glasses. Black and white objects appear black and white in spite of the fact that only colored light reached the retina from them. The color of the glasses is noticed only in looking at very bright objects.

The color disappears still more quickly when two different glasses are used, e. g. blue for the right eye, purple for the left, or yellow for the right, violet for the left, at the same time taking care that the colors are of about the same degrees of brightness in order that the disturbing rivalry of the two fields of vision may be avoided. In such cases the illusion from the judgment goes so far that the objects are supposed to be in their proper colors and it is generally impossible to say before which eye the blue, before which the purple, glass is.

The fatigue of the retina is not the only cause of this phenomenon, for the illusion occurs almost immediately and the posterior images that always follow fatigue are either very weak or entirely lacking. Moreover, the fact that we do not judge objects according to their intensity and the qualities of their lights is also not sufficient to explain the phenomenon. There seems to be another influence of purely psychological character, arising from the relation in which the sensations of color stand to one another and being a direct result of the general law of relativity. Here we have to do with a lack in the relation of the color of the glass to other colors. If we hold a blue glass before the eyes, it is only in the first moments that we have any occasion to compare the illumination of the objects seen through it with the normal illumination.

[1] FECHNER, *Ueber die Contrastempfindung*, Ber. d. Kgl. sächs. Ges. d. Wiss., math.-phys. Cl., 1860, 71.

The longer the new illumination continues, the fainter the relation to previous illumination becomes, while there is no possibility of a comparison with the usual illumination at the same time, since with use of the completely protected spectacles just described the colored illumination covers the whole field of vision.

In these experiments I made the same curious observation as Fechner, namely, that the red glasses were an exception in so far that the impression of the dominating color disappeared much more slowly and in the case of bright objects never dis-

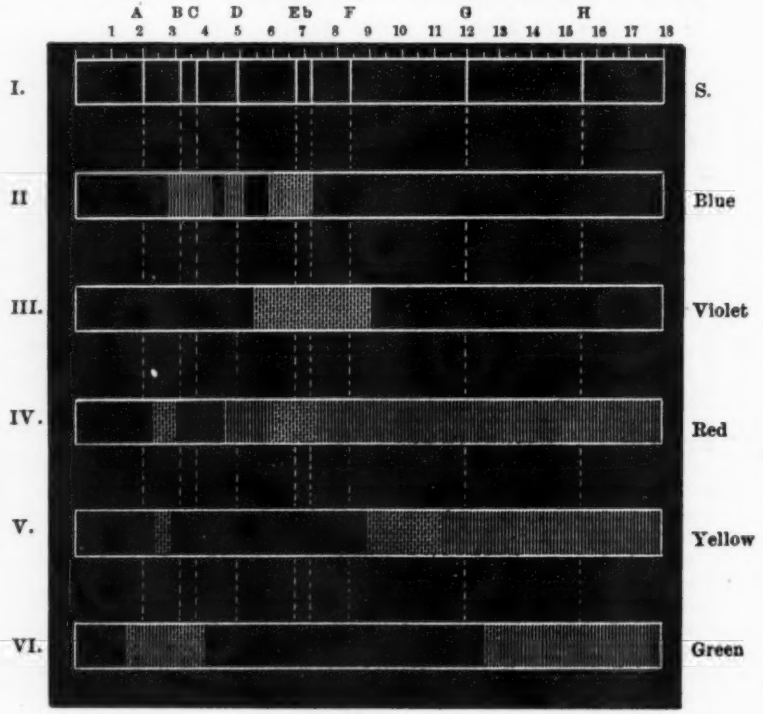


FIG. 7.

appeared entirely. Nevertheless I do not think that any higher or more enduring sensitiveness for red is present; on the contrary, I believe that the peculiarity has its cause in the physical character of the glasses. A transparent body can

possess a color from two entirely different reasons; either when it allows only rays of a certain wave-length to pass through, or when it allows all to pass through except those of the complementary color. A glass can be yellow because it allows only yellow's rays to pass or because all rays pass through it with the exception of the blue rays which it absorbs. The glasses which I used in the experiments just described were examined with the spectroscope. The results are shown in figure 7, where I. indicates the solar spectrum with some of the principal lines, and the others the spectra of various glasses, the color as it appears to the eye being written beside it. Those parts of the spectrum which were completely blotted out are shaded with parallel lines, while those that appeared faintly are represented by crossed lines. It is very clearly seen that the glasses that gave the spectra II., III., V. and VI. (blue, violet, yellow and green) owe their colors as seen by the eye to the fact that the complementary color is eliminated, while the remaining colors are allowed to pass, although some of them are weak. The red glass, however, (superficial oxide of copper coloring) has its spectrum cut off sharp before the D line and with the exception of a light gray glimmer in the place of green permits only light from $\lambda = 0.0007258$ to $\lambda = 0.0006026$ to pass. In consequence of this its effect on the eye is quite different from that of the other glasses. The others do not weaken the light from the objects to any great degree, with the exception of those in the complementary color, and consequently their relative relations of brightness remain almost undisturbed, so that we easily recognize the colors and shades. On the other hand the red glass causes the greatest disturbance in the relations of intensity of the colors of the objects. All red objects appear bright, while all those otherwise colored are seen to be much darker, often almost black. This difference is made still greater by the contrast in saturation between these two classes. Both these influences render it difficult or impossible to abstract from the red coloring and to judge objects in their usual colors, as is done with the other glasses.[1]

[1] This explanation could be tested by use of spectacles fitted with blue, green, yellow, etc., combinations of gelatine sheets prepared in the manner described by Kirschmann, *Ueber die Herstellung monochromatischen Lichtes*, Philos. Studien, 1891 VI. 543, and so arranged that these colors are pure spectral colors like the red. The result ought to show that such colors have the same effect as the red. On the other hand red plates produced by the absorption of the complementary color ought to act as the blue ones described in the text.

E. W. S.

4. *The Idea of Polish.* Contrast has also a large influence in the idea of luster or polish. I say "the idea of polish," for the polish is not directly given as sensation, but is a product of the combination of sensations.[1] According to Wundt, polish is to be regarded as an incomplete mirroring, whereby under mirroring we understand the regular reflection of light from a plane or a curved surface. Polish rests upon the reflection of the light, which, although not complete like the reflection from a mirror, (where instead of the mirror we see only the object mirrored) nevertheless is not so irregular as the usual dull surfaces. With dull surfaces the light which comes from any direction is strongly reflected in all directions, while in a mirror only one direction of the reflected rays corresponds to each direction of incidence. Between these two extremes there lies the whole series of possibilities where light is reflected in all directions, but one direction is particularly favored; these belong to the category of luster or polish. The favored direction in which the light is reflected depends on the physical conditions, on the number, the position and the size of the sources of illumination, on the form of the shining surface, but is completely independent of the eye. Therefore with even the smallest movements of the eye we see displacements of the relations of intensity. Just this fact is for us the criterion of polish. Likewise the binocular observation of the object enables us to judge of luster, since the two retinal images have their maximum intensities in different places.[2] To this we must add, in the case of bodies of a luster approaching mirroring, the influence of the doubtful fixation of the visual axes and of accommodation.

Nevertheless even in cases where these conditions are not fulfilled, where every influence of the mutual support or rivalry of the fields of vision or the movements of the eyes is removed by monocular observation with completely unmoved eye, and where the object is so far distant that the insecurity of the accommodation and convergence of the visual axes can no longer come in question, the polish is in many cases still present. The explanation of these cases has therefore to be sought in something else than the influences mentioned.

In such cases we recognize objects as polished that send out a not completely diffused light when under a single illumination; the brightness of various parts of the surface stand in such a relation as, according to our experience, cannot come from an irregularly reflecting surface. It is here a

[1] WUNDT, *Beiträge zur Theorie der Sinneswahrnemung*, 315, Leipzig and Heidelberg, 1862.

[2] On the relations of between binocular contrast and polish, see WUNDT, Phys. Psy. II. 179, Leipzig, 1887.

question of considerable differences of brightness between adjacent places of the surface; this is a matter which can be essentially influenced by contrast, especially when we are unacquainted with the source of illumination.

Bodies that completely disperse the light show no differences of intensity on approximately even surfaces with a single illumination and only moderate variations on curved surfaces; the passage from light to shadow follows in a manner completely dependent on the form of the surface. As soon, however, as we perceive greater differences of brightness on a surface of a single object than would be possible with a diffuse reflecting surface, we say that the object has polish. These differences of intensity according to the prevailing maxima of brightness are produceable not only by direct differences of intensity but also under favorable circumstances by contrast; thus in monocular perception of polish we are exposed to many illusions. For example, a gray ball of dull surface can be so painted with black and white pigments that by monocular observation and unmoving eye, it cannot be distinguished from a polished, unpainted, gray ball. Movement of the eyes or binocular observation, of course, destroys the illusion. This in the weapon which in the hands of a painter enables such startling representations of polished objects by means of pigments.[1]

5. *The Emotional Tone in Contrast.* The part played by the emotions in relations of contrast is scarce less than the usual qualitative and quantitative properties of the sensations. Lipps, indeed, regards this factor as the only psychological contrast, others being physiological.[2] Nevertheless it is easily seen that the contrast between the emotions accompanying the sensations, besides the actual emotional contrasts of the sensations, being to a certain degree dependent on it without the intensity and quality contrast being bound to it with like necessity. The sensations of light can be so chosen that they are totally indifferent in regard to their emotional tone, although they are otherwise totally unlike; on the other hand an emotional contrast separated from every contrast of brightness and quality is inconceivable.

The emotional contrast, however, although at present not accessible to quantitative measurement, determines the æsthetic effect of the colors and their combinations as a result of the simultaneous contrast of the sensations. The difficulties of establishing fixed rules for this action of the

[1] See also KIRSCHMANN, *Die psychologisch-ästhetische Bedeutung des Licht- und Farbencontrastes*, Philos. Studien, 1891 VII 362.

[2] Grundthatsachen des Seelenlebens, 273.

emotional contrast rests chiefly on the fact that all the kinds of contrast, those of brightness, of saturation, of color and of the emotional tone, each of which is itself variable, can take part in the result, all of which influences must be taken into account. It is a fatal error to judge the æsthetic effect of a combination of colors from the standpoint of the color-contrast alone. It is an absurdity to make such a statement as, "green and blue do not go well together;" it should be, "a certain green and a certain blue when combined in certain degrees of brightness and saturation make a disagreeeble impression." The color-contrast between green and blue is small, but it can be replaced by one or both the other two kinds of contrast; an example in point is the beautiful contrast between the weakly saturated and intense blue of the sky with the highly saturated dark green of the forests or with the yellowish green of the fields. The reverse of the usual statement can be made, namely, that the contrast between any two colors can produce an agreeable effect, provided the contrasts of brightness and of saturation are properly chosen. The maximum emotional effect is reached when all the three—color, brightness and saturation—are properly contrasted.

6. *Preventive Effects.* Although attention is generally turned to the strengthening effects of contrast, it is nevertheless true that the case often occurs where contrast hinders us from detecting a difference between two qualities or intensities of light. Such a case of the hindering effects of contrast is found in the fact observed by Arago and Hankel,[1] that two unlike but not very different intensities are more easily judged on a moderately bright background than on a darker one. The cause of this is not, as Fechner thought, the "reversed" contrast (the so-called light-induction), but is simply the brightness-contrast. The two small objects (in the Hankel-Fechner example two small slits) are so much raised in their brightness that they come very near the maximum of brightness, whereby their very small difference disappears.

An exactly similar experiment can be made as follows: India ink has a brightness of $\frac{1}{23}$ that of a certain white paper; lampblack has a brightness of $\frac{1}{66}$ compared with the same paper. That is, the former reflects nearly three times as much light as the latter, as can be seen at the first view when the two colors are spread beside each other in large surfaces. Nevertheless if we draw two thin lines on white paper, one with India ink, the other with lampblack, we find that they

[1] FECHNER, *Ueber die Contrastempfindung*, Ber. d. Kgl. sächs. Ges. d. Wiss., math. phys. Cl. 1860, 90.

show no difference, the contrast in brightness with the white paper having reduced them both nearly to the minimum of brightness.

Another effect of contrast is the so-called light-induction. This is, however, not to be regarded, like the usual cases of contrast, as a constantly occurring factor of every perception of sight, but as an exceptional case which occurs only after long fixation. This phenomenon has up to the present been investigated only as the accompaniment of successive and contour contrast. In these cases it is probably to be explained physiologically, as brought about perhaps by the phenomena of fatigue and subjective changes occasioned by the exertion of the organs of fixation (movement) and accommodation or by the interocular changes of pressure, or perhaps by the extension of the stimulation to neighboring parts of the retina. Whether such phenomena of light-induction occur as results of simultaneous contrast is a question that has not been answered.

There is, however, a phenomenon that at first sight might seem to be a light-induction, but is really to be explained otherwise. When we steadily fixate a bright object on a dark background it gradually loses its brightness, while the background becomes lighter. This is a necessary result of simultaneous contrast and we do not need to introduce a new property of the organ of vision. The dark background in the neighborhood of the bright object is really so dark only by contrast with the object. Even a moderately bright object illuminates not only the corresponding portion of the retina on which its image is projected, but also all the rest of the retina on account of the reflection from the retinal image, on account of the very imperfect refraction of the lens-system, and finally on account of the very imperfect opacity of the eyelids and the portions of the sclerotica and chorioidea around the pupil. Even with closed eyes remarkable quantities of light penetrate through the lids to the retina; with open eyes the amount spread over the retina must be very much greater. This brightness of the whole retina generally escapes our notice on account of the contrast with the direct illuminated portions. By long continued fixation of a bright object, however, there occurs a progressive fatigue which, on account of the impossibility of a comparison with other objects not under the same conditions, we are aware of only through the change in the relation of the intensities of the object and the background. But the more the actual intensity of the bright retinal picture decreases the less becomes the effect of the contrast on the dark background. This consequently appears to become lighter, since its illumination, derived in the ways

mentioned above, takes a more prominent part when not repressed by contrast. Exactly analogous is the case of a colored object on a dark background.

That contrast is able under certain circumstances to repress the otherwise perfectly apparent difference, is seen in the following experiment. Let a small circle be painted with a very thin solution of carmine or ultramarine; it will be of a dull pink or a bright blue color and will be quite distinct from the white background. Now, with the same color but in such saturation that it covers the white thoroughly, paint around it a ring of a thickness equal to the diameter of the circle; the circle will appear to be as perfectly white as the background. In this experiment we have to do with a simultaneous contrast in saturation and brightness; the former brings it about that we do not perceive the color of the inner circle and the latter prevents the inner circle from appearing darker than the external background. That the brightness-contrast exercises a stronger effect on the inner circle than on the external background is partly the result of the fact that the background is nearer the maximum of brightness and can be made brighter only to a small degree; it also depends on the relation of the strength of contrast and the extent of the contrasting objects.

7. *Recognition of Objects.* It is a matter of great importance to determine if the general psychological law, that we judge impressions not according to their absolute values, but according to their relations to one another, works favorably for the accomplishment of the functions of the sense of sight or not.

The duty of our sense of sight lies evidently not in perceiving absolute intensities and qualities of light, but in recognizing and naming familiar objects under the most varied circumstances, and in giving unfamiliar objects their proper positions among the familiar ones. For these purposes the brightness and the color of bodies are the most important, since we can recognize their outlines and size only by help of quantitative and qualitative variations of light.

This is readily shown by an example from common life. Suppose, when we look into a room on different occasions, we see it under continually different degrees of illumination. Every object reflects a different amount of light on each occasion. Yet this in no way affects our recognition of them. We recognize them under the most varied conditions. For recognizing them we make use of their outlines and their *relative* intensities of illumination.

The absolute intensities and colors of objects, the surfaces

of which, according to their physical characters, always reflect only a small percentage of the light falling upon them, are exceedingly untrustworthy factors of the visual percepts, on account of the extremely frequent and extensive change of illumination, and are therefore the worst means imaginable for the recognition of objects. In case, however, sensations of brightness and color are to assist in the recognition and determination of objects by means of their degrees of intensity, then the eye can measure not according to absolute but only according to relative standards, since the relation between the brightness and color of an object and that of its surroundings remains the same within considerably extended limits on account of the almost complete constancy of the co-efficients of reflection, which are valid for the kinds of light possible in common life.

If we were to estimate according to absolute intensities, the sensations of brightness would be of value for the recognition of objects only in so far as they provided an easy perception of the outlines of the objects and their parts. Every variation of illumination, however, would change essentially not only the whole picture of the objects in the field of vision but would also disturb and distort the relations of brightness of these objects to one another up to the last degree of indiscernibleness.

Since we are, on the contrary, under the greatest possible abstraction from or removal of the recognition of absolute differences, and are provided with the ability to recognize and to estimate not intensities themselves but their relations to one another, therefore we are placed in a position to recognize objects through the relations of brightness of their surfaces in as far as the co-efficients of reflection remain constant by variable illumination.

It is to the law of relation that we owe the possibility of carrying out this chief function of the sense of sight, namely, the recognition and the determination of the objects of the external world. Indeed, we can say "contrast" in place of "law of relation;" for contrast is merely the expression of our conviction that the same intensities of the stimuli possess different worths as sensations according as they enter into various relations to other impressions; or, in other words, that the difference of two sensations cannot in all cases be referred to the difference of the stimuli which cause them, and that therefore from the likeness or unlikeness of sensations and ideas we cannot judge the likeness or unlikeness of the objects unless we take the whole contents of consciousness into consideration.

REPORT ON AN EXPERIMENTAL TEST OF MUSICAL EXPRESSIVENESS.

By Benjamin Ives Gilman.

During the latter half of last March I sent to a number of residents of Cambridge and Boston who are interested in music a circular of invitation, from which the following sentences are an extract:—

An Experimental Concert.— May I ask your aid in an attempt to make an experimental contribution to the question so much debated regarding the power of music to awaken definite ideas and emotions in the listener. It has occurred to me that some results of value might be reached if a careful selection of musical fragments, to which a definite expressiveness has been attributed, were to be performed in the hearing of a number of persons interested in music, who should previously agree to set down independently of each other the impressions they receive therefrom. Should a sufficient number of those to whom this notice is sent signify their willingness to join in such an undertaking, I propose to give what may be called an experimental concert, on some evening to be hereinafter selected. A lack of theoretical knowledge of music need in no way be regarded as a disqualification for the task proposed. On the contrary those who *feel* rather than *understand* music are to be considered to be the best audience for such a purpose. An interest in the purely musical aspect of a composition might hinder rather than help that imaginative grasp of it which it is here desired to test.

I propose to obtain the listners' judgments in the form of answers to a question prepared beforehand on each of a number of musical selections, which appear in a definite way expressive either to myself or others.

The questions will be numbered and each of the listeners will be provided with a note-book, in which he will be asked to jot down his reply (to be numbered to correspond) without consultation with others, during a pause after each selection. Each listener will be asked to write down the name of any one of the fragments which proves familiar to him.

The undertaking is so entirely novel that it is difficult to predict its outcome, but I am sure there are many among us whose imaginative interest in the art of tone and power of giving this interest expression are capable, if the right opportunity be offered, of throwing a valuable light on the vexed question of musical expressiveness.

About thirty persons were kind enough to accede to this request, and on the evening of the 29th of April the experiment took place in a parlor in Cambridge.

The instruments used were a grand piano (from the Mason & Hamlin Co., Boston.) and the violin. The interpretation

of the programme was intrusted to three well-known musicians of Boston, Mr. Charles L. Capen (piano), Miss E. M. Yerrington (piano) and Mr. A. van Raalte (violin). I am glad to express my appreciation of the interest in the undertaking displayed by these artists, and my thanks for their skillful coöperation.

The whole company, performers and audience, began the evening in a very sceptical frame of mind regarding not only the value of any data which might be obtained, but even the possibility of carrying out such a test. The result belied our forebodings. The method of inquiry proved a practicable one, and there was, I think, a general feeling of surprise among the listeners at the amount of booty rewarding their determined efforts to capture the suggestions of the music played. I may be permitted to express my personal belief that only very rarely indeed would it be found possible to enlist in such a cause as much ingenuity, candor and good-will as was shown by the subjects of this experiment. It was expected that several musicians by profession would be among their number, but as it turned out the audience consisted entirely of amateurs. A large minority, if not a majority, of these were without special skill on any instrument; a few were distinctly non-musical in the sense of having no marked endowment of musical ear or memory; but there were none present, I think, who were not capable, at least at times, of enjoying and feeling music deeply.

The work of the evening consisted in obtaining answers to fourteen questions based upon thirteen selections of music, one being the subject of two questions. Nearly all of the pieces were played more than once, some several times, and although they succeeded each other almost without intermission, except for putting the questions and making necessary explanations, the experiment lasted without any relaxation in the interest of the participants from eight o'clock until about midnight. Twenty-eight notebooks were the result, sixteen contributed by gentlemen and twelve by ladies, the former being indicated in the transcription below by Roman, the latter by italic capital letters. Each listener replied on an average to about three-quarters of the questions. The contents of the notebooks here follow (one written in German is translated) appended to the several questions, with each of which is given the particulars of the music to which it refers and the expression of opinion on which it was founded. Two of the selections were fragments of elaborate concerted compositions, and as it proved that the suggestion of these, which alone could be given by the piano and violin, did not afford sufficient basis for judgment, they are not here reported upon.

As one of them was the subject of two questions the following transcription consists of the replies to eleven questions on eleven different selections. These are all either piano compositions or melodies written for the voice with or without accompaniment. The notes are given as they were handed in at the close of the evening, with changes (to carry out the intention of the writer either evident or since ascertained) in perhaps not more than a score of words in all. The selections are understood to have been unrecognized unless the contrary is expressly stated.

Question I.

Give any image that is strikingly suggested to your mind by the course of the following piece.

Beethoven. Pianoforte Prelude in F Minor. It bears no opus number, but in the Breitkopf and Härtel edition of Beethoven's Works, is No. 195 in Series 18, "Kleinere Stücke für das Pianoforte."

To the writer its character is that of an unending contest with an opposition that bars every advance. It is an attempt to hew a way through adamant. We could fancy ourselves listening to the tireless dialectic of a mediæval schoolman on some transcendental thesis, or even admitted to the mind of a melancholiac eternally resenting miseries eternally visited upon him afresh. Dry and gloomy energy doing doughty deeds to no purpose is to me the burden of the piece. Piano solo.

Answers to I.

A. The swaying of the treetops in a moderate wind; weird songs are sung beneath the trees.

B. A country church appeared to me; the music formed the chimes; the surrounding scenes were grave or gay as the music became slow and soft or fast and loud. As it died away a funeral train seemed passing.

C. No image. Technique (not of performance but of composition) entirely covers up the æsthetic effect. I cannot help being lost in the sequence of the strain, especially on an instrument of percussion like the piano.

D. At first, organist seated at organ in church, then a change at end to twilight; a large hall; a man who has felt sorrow, yet feels the grandeur of life above all, improvises; a love sadness.

E. Plunge of a torrent in the woods; then children's feet dancing as the key changes; sunburst. Thenceforward the piece gets more dramatic, forming a sort of tumultuous dialogue or inward dilemma of affirmation and negation. It rolls on some practical moral decision, and with moments of peace or weary diversion it ends in a sort of forgetting calm without particular triumph.

F. A hymn of thankfulness.

H. Persistent struggle with rather mild difficulty, e. g., walking through a wood with thick underbrush.

I. Chime of church bells; bright, sunny morning; gathering to church; in church; entry of minister; hushed; minister rises; ready for service; last stroke of chimes.

J. The rolling up of breaker after breaker on the beach with the sound of more distant rollers in the lulls; or the dying away of a storm.

K. This is a fugue. Fugues always suggest to me the beauty of organism, the universal not being built out of an accretion of particulars

but revealing itself in subtle relations among them. The complexity of law. The essentialness of sadness to happiness.

L. A great strife against something; a final conquering of this something and then rest. This strife seems to return at times and is then quieted; finally, near the end a burst of it and then the quiet closing bars.

M. Suggests a life toiling on through disappointment and struggle, until at last peace comes, a peace of which there had been moments of anticipation. Not a brilliant or a prominent life.

N. The resolute self-possession of the process that is going on suggests at once something very much alive, very free—a nature force in full possession of its own world: Sie entlässt sich frei, ihrer *selbst ganz sicher*, says Hegel of the *Idee*, when it passes over into *Natur*. I have a sense that a water-process would be the scene most naturally suggested. Scene, however, not complete, but waves on water most probable.

O. A rather distinct idea of a workman making something by strokes, as a smith. There is also a feeling that he is in a lazy mood, as if the afternoon sun were streaming in. The work is pleasant.

P. Church; opening voluntary. Religious cheerfulness. A religious dance; measured movement of hands. Or, somewhat, a brook tumbling along over a stony bed. The suggestion of a yearning.

A. (Bach.) A ship approaching end of voyage; all tension; haven.

C. It (the piece) seemed to me to embody the progress of a mountain stream on its course from the hills to the plain, flowing among rocks over many obstacles, under the forest trees, with the quiet and deep repose of the wild wood pervading all. This was the only image that occurred to me. The intensity of the stillness of the wood was most prominent.

D. Persistent effort, resulting in serene progress.

E. A perpetually struggling bird, flying up and beaten back by the wind.

F. Beating of the waves upon the rocks in the receding tide.

G. Storm wind; agitated sea; dashing on rocks or through pines; increasing, then gradually subsiding. A rock-bound coast with weather-beaten woods, mostly pines.

Spiritual vision: Strong emotion; unrest; doubt; gradual peace, though not joy.

H. The last part makes upon me the impression of a scene of farewell, and I seem to see the departing friend disappear beyond a hill.

I. Dark clouds; storm. An old German church with a suggestion at the close of a funeral service over some great and heroic character. A feeling throughout as of a strong resounding sea against a frowning coast.

J. A controversy or argument between a man and a woman, ending in a great peace.

K. The incoming tide dashing on the rocks, with intervals of quiet ebb.

L. Church music; offertoire; also, organ playing while waiting for a wedding party; cheerful, and not too joyful; serene; also, the suggestion of hearing the organ playing inside, while outside, in the summer. (Bach.)

Question II.

The two melodies to be played are said to be in a certain respect opposite in character. What particular form of contrast, if any, do they in your opinion embody?

First eight bars of "O, mio Fernando," aria from the third act of Donizetti's La Favorita, and first five bars of "Durch die Wälder," aria from the first act of Weber's Der Freischütz. In "The Power of Sound" (London, 1880), p. 108, Edmund Gurney applies to the melody from La Favorita the words "flaccid feebleness," and to that from Der Freischütz the phrase "serenely and lastingly fair and strong." Violin solo.

Answers to II.

A. (1.) An expression of sadness and love.
(2.) Light-hearted, triumphant affection. Or,
(1.) Liebesweh.
(2.) Freude.

B. Despair; a plea for mercy versus relentless triumph.

C. Distinctly opposite; something like yearning and satisfaction (in the same range), or doubt and assurance (in the same range), or anxiety and relief.

D. (1.) Resignation.
(2.) Gaiety with a touch of something else. Or,
(1.) A gray haired woman sitting at a window.
(2.) Some touch of laughter.

E. (1.) Plaintive, looking to past.
(2.) Joyous, looking to future.

Number 1 seemed to me at first to express a noble *resignation*, but later I was uncertain whether *longing* was not more strongly there.

F. (1.) A farewell, or a regret.
(2.) A greeting, and an expression of delight.

G. (1.) Plaintive.
(2.) Joyous.

H. The first piece is poetic, melancholy, moonlight music. The second joyous, though with a certain seriousness, and is full of sunlight.

I. The first melody was sad, depressed, longing, regretful, *relaxed* body. The second was joyous, buoyant, expectant, up and doing.

J. Sorrow without hope. Sorrow with hope.

K. (1.) The daughter of a Teutonic chief pleading with a Roman general for her father's life.
(2.) Triumphant leadership; command. Contrast: The earnest seeking of an essentially feeble nature, and the joyful success in attainment of a nature that is strong.

L. There is a sense of unsatisfied longing about the first. The second seems vigorous and full of hope. The first is dependent, the second independent, in general effect.

M. First selection reminds me of moonlight on the ocean; calm; a small boat rowed slowly. Second suggests a sparkling breeze; in a sailing boat. (N. B. I am fond of the water.)

N. The well known quotations from two poems of Browning *at once* suggest themselves as characterizing the contrasted moods.

(1.) "That was I you heard last night,
* * * * *
Serving most with none to see."

(2.) "So, I shall see her in three days
And just one night, but nights are short,"
* * * * * *

[The two poems are those entitled, "A Serenade at the Villa," and "In Three Days," in the Dramatic Lyrics.]

O. Discouragement; confidence. Doubt; gay, nonchalant purpose. The contrast is not very definite in my mind; strong, but vague.

P. First melody, plaintive; gently sad, as of a lover leaving his

mistress, or a poet who is conscious of missing his ideal, or a girl leaving home to go to school. Second melody, vigorous cheeriness; manly satisfaction; a certain joy and springiness.

A. (1.) Pathos; sadness with hope.
(2.) Allegro; cheerfulness.
C. (1.) Self-controlled emotion.
(2.) Impetuosity.
D. (1.) Looking backward with sadness and regret.
(2.) Looking back on happiness.
E. The first suggests a wandering about in dark places. The second, a buoyant spring upward into regions of light. The shallowness of both strivings is felt.
F. The first expresses hesitation. The second, resolution or achievement.
G. First, a petition, as of one doubtful of the favorable response, but beseeching a hearing; not devotional, but human, as of a lover and his mistress. Second, a reply—not of a love-lorn maid, but cheerful and slightly upbraiding.
H. (1.) Evening peace.
(2.) The freshness of morning.
(1.) Quietude of soul.
(2.) An awakening to new energy.
I. (1.) Sentimental.
(2.) Lively.
J. The first is seeking, the second expresses attainment.
K. A contrast as if a mood of somewhat sad meditation beneath the sighing pines were cast off and serene joy took its place.
L. (1.) Serenade; hopeful.
(2.) Window opens, all is joy; he feels she cares for him.

QUESTION III.

What is the main impression produced by the following passage taken as a whole?

Beethoven. Piano-forte Sonata in D, opus 28 (often called the Pastoral Sonata, but, it is said, without warrant from the composer). Fragment of the allegro, beginning with the 77th bar, and ending with the 125th. According to Edmund Gurney (Power of Sound, p. 169), this passage "affects the inner sense with a compulsion, a concentrated passion of movement, so overpowering that I scarcely know its parallel in music; the four bars break in the middle, making the swing of the motive, as it recurs, seem more than ever resistless." The allegro was played from the beginning up to the 136th bar, the attention of the audience being especially called to the passage remarked upon by Gurney. Piano solo.

ANSWERS TO III.

A. Joyful contentment.
B. The piece brought to my mind a girl half talking, half singing to herself, ending with a careless laugh.
C. Very vague; but something like the joyous feeling of out doors, with its invigorating and cheering influences.
D. It suggests the opera; the orchestra works to a mild climax; not the grand climax of the whole. A woman sings one of her *first* songs; a touch of feeling ending with the customary runs.
E. Noble joy on a terrace, eighteenth century, people in pearl color and powder dancing it; then the piece loses that date; the joy appears

based on an assured good impatiently looked for in the rapid running passages, and the reasons of the certainty of its coming rather triumphantly laid down in the staccato thumps.

The above describes the whole piece, not only the termination.

F. The coming of spring.

G. Suggests a melody in one of Sullivan's operas, perhaps Iolanthe: "I heard the witch remark, etc."

H. Impression very slight; mild progress and success; runs, to me, are meaningless.

I. Song chorus; jaunt on the cars; singing to the beat on the rails; no deep emotion.

J. Gave me a feeling of light-heartedness, such as one in perfect health has in the early morning of a beautiful day; the joy of life and nature.

K. Renaissance work. Trivialness of surroundings; flounced dresses, hair powder, coats with long skirts silk lined, elaborate walking sticks, —and human hearts beating, life real in it all.

L. This (or something that suggests it strongly) is familiar to me, and as I have entirely material associations with it, I cannot disconnect them from the music. As far as I can do so, it seems at first a little trivial, becoming then more serious, with occasional outbursts of the trivial side.

M. It puzzles me. The impression is filled with charm, but is very difficult to analyze. Suggests something slightly frivolous. A comic opera?

N. Beethoven "Pastoral Sonata." My impressions are very old and personal as to this sonata. I never found especially "pastoral" associations, as such, in the first movement, although I always imagine myself in the open air, under blue sky. But that is arbitrary. The passage in question has purely religious context otherwise in my feeling; the climax of a moment of cheerfully adoring resignation, voluntary abandonment of finitude, with a certain insistent and repeated delight in laying off, as it were, the clothes of one's soul before taking a very jolly flight into the blue.

O. Nothing clear.

P. Dance of village young men and maidens; pleasant or gay responses; mild abandon.

A. A child learning to walk: Step high; step low; faster; ha-a! run!

B. Serene confidence.

C. Undefined.

D. The joyful consent of many.

E. Known: Beethoven. A wavering between two desires, each of which is worthy; now one is stronger, now the other, and the decision comes nearer and nearer. It is almost reached when the steps leading to decision all are shattered and have to be retraced. The conclusion of the whole matter is a decision inconsistent with the premises.

F. A vague impression of regret.

G. No clear impression. The first half brought a remembrance of a peasants' fête in Brittany; the last half, nothing.

H. The joyful uplifting of an oppressed soul that feels itself released from depths of anguish through faith in a kind, heavenly Father.

I. No impression other than a musical one.

J. A demand; a bitter disappointment, concealed by gaiety and nonchalance sometimes, but ill concealed.

K. Rocking in a boat on a dancing, sparkling sea; surroundings cause a happy state of mind.

L. No impression.

QUESTION IV.

The following music has been said to tell a certain story. What dramatic suggestion do you find in it?

Chopin. Ballad No. 2, in F major, opus 38. In his recent essay, "Die Musik und ihre Meister (translated under the title of "A Conversation on Music," N. Y. 1892), Rubinstein writes (p. 10): "Is it possible not to call instrumental music a language? Of course, if the first movement be rendered merely in a lively tempo, the second merely in a slow tempo, and the third merely in a spirited tempo, the executant feeling no necessity for further expression, then we might call instrumental music non-expressive," and regard vocal music as alone capable of real expression. * * * Another example: The Ballad in F major, No. 2, of Chopin. "Is it possible that the interpreter should not feel the necessity of representing to his hearers: a wild flower caught by a gust of wind, a caressing of the flower by the wind, the resistance of the flower, the stormy struggle of the wind, the entreaty of the flower, which at last lies broken? This may also be paraphrased: The wild flower, a rustic maiden; the wind, a knight; and thus with almost every instrumental composition." Piano solo.

ANSWER TO IV.

A. First hearing. Two lovers are on shipboard and their happiness is interrupted by storm and eventual shipwreck.

Second hearing. Two happy lovers are sailing over smooth seas, the ship is attacked by pirates, who are beaten off. A fierce storm arises, the ship bearing the two lovers is destroyed, and after the storm the sun shines again upon the sea, now somewhat calmed.

B. The piece naturally suggested a *murder*. It opens with a picture of the assassin creeping *slowly* along and you hear the shrieks of his intended victim when he is brought face to face with his slayer. Here the music, now *shrill*, now *deep* and *low*, seems to mingle cries and groans as the deed is committed and the man finally dies. The assassin slowly crawls off again and the lighter tones that are introduced seem to be the ordinary events of life passing on about him, producing much the same effect as the knocking on the gate in Macbeth. At the end the murderer is by himself and the last notes suggest regret entirely unavailing over what has happened. It ends with wild remorse.

C. (General impressions in succession.) Devotional; storm; gaiety; stormy; gentle again.

Absolutely no definite dramatic suggestion. If story known could easily be made to fit, but without that no single suggestion, not even perhaps definite emotions.

D. It suggests no definite story. A quiet life interrupted by some sort of passion, back again to the old theme, with something richer in the harmony of the old, something gained from the interruptions.

Second hearing. I do not fully understand the sadness of the first part, that becomes an undercurrent in the first interruption only to reappear. It might be called a romance. In some way it suggests George Sand and Chopin. Mr. —— (another listener) tells me his story and I have now the definite picture I lacked.

E. The thumping and haste of the latter one-third or one-half were nothing to me but *intolerably disagreeable* noise, quite meaningless. The first uniform segment of the piece was delicious noise, of which the only dramatic suggestion was the passage through life of a rather rich-mindedly sober and patient sort of man, with one leg shorter than the other. His inner gravity and modesty seemed connected with his lameness. At one point he tumbles off the bank into the water and then, *bang!* is caught in the whirlpool rapids for a long time until he

gets out rather wet; after which nonsense and noise, but for the short gleam of sanity at the very end, which is sweet but irrelevant.

On second hearing arbitrary character of noisy half more infernal than before.

F. Pastoral peace; sudden alarm, (why the lull here?) then conflict, bells, fire, night; and rest.

G. The only part which I recognize is a thunderstorm in the middle of the piece.

H. If I had not been told I do not think I should have felt any story; as it is the piece suggested successive batches of dolls out for a morning walk; they meet soon naughty boys, who rip them open and make the sawdust fly; wild war-dance of boys; one lone doll going home.

I. Lullaby; mother; baby; cradle; rocking; love-gentleness; good night. Changes to thunderstorm; fear; rain ceases; lullaby again. New theme thrust into the picture; leads up to a strong emotion; not quite tragic; hope of future for child. Now grows tragic, the sky of hope for the future clouded; trouble, sorrow; all may be a dream of the child's future by the mother; confused gathering to a climax; breaks; envoi.

If a *dream*, it (envoi) is a sad good night; but rather it is now the narrator saying "So it was, and so he died," "So it ended," "That was the end of him."

Played a second time. Lullaby as before, storm ditto, all as before.

J. Peace and quiet—sudden terror—violence, confusion. At the last a climax, as of a violent death, succeeded by silence, a hush. The main impression one of great *contrasts*, sudden changes in scene. Human strife breaking in upon human peace and happiness, or upon the quiet of nature; the whole rising to the climax; increasing rapidity and intensity of action; the catastrophe; the end.

K. A bare heath, or stretch of sand beach; she remonstrating with him, who is going. Then a battle, she seeking among the dead and wounded. Dido and Æneas.

The general form of which the preceding are the content would be: The sad calm of monotony broken in upon by a catastrophe whose succeeding waves overwhelm the soul.

L. The story which first suggested itself and which I can't get rid of, although it seems in places inconsistent, is of two persons at first amicable and then quarreling, finally becoming reconciled only to quarrel again. This seems (is) too trivial for such a magnificent thing. As to the musical enjoyment of this selection, it is very great. The more I think of it the more artificial my answer seems, though I have no substitute for it.

M. Extremely beautiful, especially at first. Early part suggested monastic life, as it should have been, in the middle ages. Then war sweeps over the country and demolishes the monastery. An effort is made to reconstruct the old life (whether it is a tale regarding an individual or a group I do not know), but the attempt is not finally successful. The life then becomes confused, mingles with the crude movements of cruel times, and ends in physical or moral battle. At the last moment occurs a recollection of earlier peace.

N. (My first sketch on first hearing thrown out on second hearing. The first was due to a false reminiscence.)

The hero of this bit of tragedy is in much the same world as Poe's lover in

"Thou wast that all to me, love
For which my soul did pine,
[A green isle in the sea, love
A fortress and a shrine,
All wreathed with fairy fruits and flowers
And all the flowers were mine.

Ah, dream too bright to last!
* * * *
No more, no more, no more,
(Such language holds the solemn sea
To the sands upon the shore)
Shall bloom the thunder blasted tree,
Or the stricken eagle soar.

And all my days are trances
And all my nightly dreams
Are where thy dark eye glances
And where thy footstep gleams,
In what ethereal dances
By what eternal streams."]

(The poem is called "To One in Paradise.") Only he struggles more fiercely for the lost love, has a much more Titanic determination to win her back—but fails.

O. No *coherent* story suggested. At first it seemed as if a young girl were living a quiet passive life. Then an external misfortune, a war perhaps—not a gradual passion of her own—came upon her. The rest seemed a sort of pursuit and effort to rescue the victim. This involves the sacrifice of the old life, which at the time seemed dull and sad, but now seems beautiful.

On second hearing it seems there are three passions or tendencies, a solemn and awful one beside the exciting one. But I can't define what they are.

P. Summer: zephyr followed by storm.
Or hymn within a convent, and a surging crowd or a battle without.
In the soul: conflict with intervals of peace.
A raging soul listening to a distant church-service.

A. Fête champêtre. Children's minuet—in old fashioned dress.
A fierce storm; thunder, lightning; consternation, dispersion. End of dance, gravely.
After interval—a stately procession or polonaise, by the same company grown. A storm of warring passions. Reminiscence of first scene.

B. Disappointment, spirited endeavor, success.

C. The piece suggests to me the following dramatic situation: Firm, calm, resigned acceptance of fate or future by an earnest nature capable of deep feeling, though in opposition to desire; met by equally decided but stormy resistance and expostulation, on the part of an unprincipled and ardent nature, which is unsuccessful at the end, though unconvinced. Love would be presumably the theme.

D. A funeral march or dirge for a young girl chanted by her girl friends. A sudden contest and strife. I thought of Ophelia's death and Hamlet's struggle with Laertes. After long strife the dirge is repeated as if thought centred again about the dead girl.

E. A life capable, sensitive, forceful, recognizes very early its own possibilities for good and evil. The life is a fair one outwardly and the world judges it right in calling it blameless; but the world cannot see the inner mental and spiritual working.

(I.) The soul itself sees beneath its own outer and even inner calm; and recognizes that there is some element here that will work harm; it sadly feels its own powerlessness, or weakness of will to prevent the harm.

(II.) Then the thought of temptation with its horrible significance clashes on the soul; but the anguish caused by the thought repels it at

first, and keeps it at a hovering distance, yet the soul knows that the temptation will return. In this first period the soul knows that, mentally, it is stronger than its foe; but in the next stage, the soul is weakened and does not resist; it welcomes all the horror that it hated before and yields fully to the delight of the sin,—all this mentally.

So at the end, as in the beginning, it still appears calm and pure in the judgment of the world.

F. It is the story of a life beginning with a happy peaceful youth, suddenly broken in upon by excitement and adventure, ending finally in a return to the quiet, uneventful life of the early days.

G. First hearing. Scene, a square in a foreign town; cathedral spire; bells; procession of priests; organ music; enter church; sudden inrush of soldiers and people; gaiety, dancing; hush! reappearance of priests bearing a bier; bells and organ again. They pass on and new scenes, of varying character, occur; but all en masse and all in the same square. No *direct personal* drama of individuals.

Second hearing. A something ominous! War! Storm! a gathering in frightened groups. Ah! the bells again. . . . .

H. I hear two antagonistic elements in conflict—the bells of cloisters inviting to tranquillity, and without, the raging storm and strife of men for earthly gain, where the strong conquers and the weak succumbs.

I. The opening movement naturally suggests a gondola, and that of course Venice with its thousand associations of intense and varied life and adventure; but it was Venice that suggested whatever images arose in my mind and not the music especially. Naturally Browning's "In a Gondola" was thought of; also a storm,—with wreck,—and returning calm, but the sullen calm of subsiding storm.

J. Peace and war; peace finally. The first part a mother soothing her child.

K. No story is suggested; but expostulation and sometimes entreaty opposed to a vehement and impulsive expression of something desired to be done at any cost.

L. Practising church music; old chants and quiet chorals not too grand, broken in upon by thunder-storms; boys' voices, and almost relief.

Question V.

How would you describe the general mood which the following music is fitted to incite, or the atmosphere which seems to pervade it?

Beethoven. Pianoforte Sonata in E, op. 109. Andante Molto cantabile. The theme alone without the variations.

It suggests to me a mood of devotional meditation (Andacht).

Piano solo.

Answers to V.

A. I am still too much under the influence of number IV. to be affected by this number.

B. Doubt; hesitation.

C. Resignation.

D. Peaceful; but sadness in it.

E. Pensive; not passionate—and grave; not regretful. (Nothing more determinate!)

F. Prayer.

G. Proceeds from a placid mood in the presence of the sublime.

H. Religious.

I. Devotional scene; not *very* religious, but dignified.
J. Seriousness, solemnity, thoughtfulness, religious feeling.
K. Reverent, joyful worship.
Strasburg Cathedral; a procession passing along the nave; a choir-boy swinging a censer turns his face and looks at the spectator.
L. Somewhat religious, though it has a shade of vague unrest in it.
M. Religious; suggests some German church music.
N. No impression worth noting beyond a general atmosphere as of a calm introduction to a dignified ceremony (?)—This interpretation seems doubtful.
O. Not sure:—thought still about the former piece. Is it religious peace and resignation?
P. Tender seriousness.

A. Religious expansion; grateful worship of a full free heart.
B. Seriousness of life.
C. Tender religious melancholy tinged with a sense of pathetic pleasure.
D. Placid retrospect.
E. Known. A mood of comfort and endurance born from sorrow.
F. Retrospection.
G. Devotional; religious.
H. Longing after a higher life.
I. Hoch, heilig und hehr.
J. A generous and complete nature.
K. Self control and the quiet happy feeling that follows success.
L. A restless person waiting for some tardy arrival, trying to forget himself in writing out some serious music.

Question VI.

Do you detect anything not commonly depicted in music that finds its expression in this melody?

Mozart. Nozze di Figaro, Cavatina of Barberina "L'ho perduta" in the 4th act. According to Gustav Engel (Aesthetik der Tonkunst Berlin 1884, p. 134) Mozart has introduced *comic* traits into this air.
Violin and Piano.

Answers to VI.

A. A fading and wilting flower.
B. The wind crying through a knot hole in the attic, as I have heard it in an old tumble down house.
C. A very sad humor. The impression of oddity and pathos is distinct to my mind.
D. Indefinite.
E. Sort of rustic uncouth unwillingness ending with consent.
H. Seems to ask a question.
I. She is told or discovers that he is not true; someone intercedes and says it may not be so. She with dignity but with firmness tinctured with deep grief resents the conduct and is turned against him. (Is not very definite in its suggestion; not clear.)
J. Anxiety; weakness; as of a crying child; relief, as though the mother came.
K. Not uncommon in music. Disappointment; "Well, never mind." "Men must work and women must weep."
The pathos of monotony half felt in the midst of the commonplace.
M. Plaintively beautiful. Yearning aspiration? Cannot think of any unusual feeling expressed.

N. The only impression would seem to me no more uncommon than any other. It is an impression of someone seeking in hope and sadness mingled for some lost thing—whether a lost child or a lost latch-key, I can't say. The search grows more painfully despairing and ends in giving the whole thing up. Even so I have looked in vain through all my pockets for the last nickel and found it not. The search is one made in grief, but there is no high tragedy.

(Certainly not consciously recognized in any way, nor remembered.) Parenthesis added after learning real nature of piece.

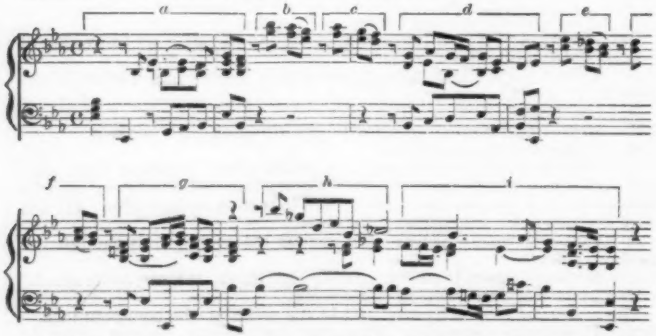
O. Nothing clear.

P. Nothing, except, perhaps, a moan; yet the like is common in Mozart, e. g., in his Tito.

A. A sunny Mark Tapley-ism.
B. Anxiety; gentleness.
C. Indecision.
D. Questioning.
E. An every day customariness.
G. Seeking, doubt, questioning, despair.
H. A conversation of lovers in questions. Teasing.
I. Entirely engrossed by the music; no images or impressions.
J. Blindness.
K. The sweet, stern character of a noble woman.
L. A little child talking to her favorite doll, telling her how much she is loved and begging for an equal return of the love; then singing her off to sleep.

Question VII.

Händel's air, "He was despised and rejected of men," from the Messiah, is generally regarded as a musical expression of great sadness, if not dejection. As the successive phrases of its introduction are now played through, indicate which contribute chiefly to its character of melancholy, and why they do so.

The notes of this selection were exhibited on a placard in sight of the listeners, the successive phrases being lettered as follows:

Of this fragment Gurney writes as follows (Power of Sound, p. 273): "In the opening to 'He was Despised,' the pathetic effect in the sixth

bar of the G flat or minor third of E flat, as distinguished from the major G natural, which might have been used, is instantly recognized." Again (p. 329.): "The effect of the minor intervals in this melody was noticed in the chapter on Harmony; but the character is really set in the opening six [five] bars of the piece, before any such interval has occurred. Here the motion, grave throughout, owes its character of absolute dejection mainly to the two groups of three descending thirds in the middle; these receive a special intensity from the accents falling on the second of the three chords; and the pause after the first group emphasizes the effect of the reiteration, as though the power of movement were gathered again only to sink to a still lower depth of depression. . . . . These features present a clear affinity to physical movements of drooping and collapse." Violin and piano.

ANSWERS TO VII.

A. It seems plaintive and supplicating to me, but not necessarily very sad; phrases *h* and *i* are perhaps sad.

B. The piece gives a *general* impression of *sadness*, rather than dejection; of mourning that the past is past, rather than of fear for the future or regret for the present.

C. The extreme dejection begins with the G flat, continuing through the next bar.

D. Phrases *e*, *f* and *h* contain the element of sadness; *i* is full of feeling; *i* and *a* and *g*, have the human feeling; the spiritual element comes in the *d* and *c*.

E. Phrases *c*, *f*, *h* (especially), pain! phrase *i* gives way.

F. All through, great dejection, especially *b c*, *e f*, and some notes in *h*; but (query) was it not the wail of the strings? Would it have been so on the piano alone?

G. Sad throughout, but especially in *a*, *d* and *g*.

H. To me the dejection is continuous.

I. The dejection comes first at *b*, next at *e*, but a trifle less here than at *b*. The climax is on *h*. Phrase *i* disappoints me as not fulfilling what was required to complete the dejection. The G flat in *h* is the decisive note.

J. The two falling cadences like sighs, and the last bar *giving way* to sadness.

K. The dejection comes in the first place from the abruptness produced by the short rests. The incompleteness of the short phrases *b c*, *e f*, suggests the cutting short of the life, whose grandeur is felt in the beautiful harmony in *a*, *d*, *g*, and *i*. These last form a connected whole of which the previous are an interruption.

In the second place the change of key in *e* and *h* adds to the sense of normal development interrupted; and this is further carried out by the minor in *h*.

The self-restraint produced by the pause between the second and third notes of the violin [which was played as above indicated, and as the phrase is sung, and not as it occurs in the introduction] adds to the pathos.

Phrases *a*, *d* and *g* represent the great soul struggling on its course; phrases *b c*, *e f*, *h*, represent circumstance, fate, the cutting off of the soul's course.

L. Close of phrases *a* and *g*, to some extent *b* and *c*, *d* and *e*. Most of all to me phrase *h* and beginning of phrase *i*, which seems to express an utter hopelessness. Of course, as a whole, the air is very sad.

M. Phrases *d* and *g* are spoken words of sadness; the phrases played by the piano alone are sighs; the halting notes at the conclusion are the culmination. The introduction of successive flats evidently causes a part of the effect.

N. The "dejection," apart from the tempo, is not marked in the first phrase, appears, however, in *b*, and in the contrast between the high and low phrases. The concluding phrase is one of a certain sorrowful consolation. Nor is the dejection anywhere without its accompanying and contrasted consolation, which appears in the phrases low in the scale as a certain gently solemn offset to the grief. In this contrast lies the chief art.

O. Decidedly at *h*. The rest is also sad, but *h* especially so; it is piercing. Dejection is not so much expressed in *h* as sadness.

A. Phrases *a* and *c*, deep sadness; *d* and *b*, tender compassion; phrase *h* is heroic; phrase *i*, exquisite pity (love).

B. The most touching, the three last measures.

C. Impression of dejection begins in *a*, increased in *d*, still more in *f*, complete in *i*; lost more or less, or perhaps I should say diminished, in the intermediate phrases.

D. The passage *d* expresses dejection, discouragement, most powerfully.

E. The whole selection contains profound dejection; the theme is announced in *a*; *b* and *c* are the sorrowful suggestions of the sufferer that there may be better things in store, yet even here the downward phrasing implies that hopelessness brings the hope immediately downward; *d* marks the better conclusion of the sufferer; *e* and *f* are new suggestions of hopeless hope, and *g* the heartbroken answer that the bitter fact remains; *h* is the admission of the counter-soul that pain is, after all, necessary; the final conclusion is one of the deepest depression.

F. Phrases *b* and *c*, and *e* and *f*, seem to express sadness, yet their expression of it would seem incomplete if they did not lead up to the phrases *d* and *g*; but *h* and *i* would, to me, separately express the tone of the whole fragment.

G. The poignancy of the sadness increases with each phrase, and culminates in the last heavy dejection and gloom at the end — a gloom not of anger, but of sorrow accepted.

H. The expression of deep sorrow and abandonment is accentuated at *e*, and, to my feeling, culminates in *h*, but after all, is very likely a resultant effect.

I. The phrase *a* is not conclusive, but taken to *d*, the expression of dejection is complete, without the other phrases.

J. Phrase *h*, beginning with the fifth note.

K. Impression of sadness through the whole piece.

Question VIII.

What single adjective best expresses to your mind the general impression of the following music?

J. S. Bach. Well-tempered Clavichord: Prelude in E flat minor. In the "Conversation in Music" (p. 5.), Rubinstein writes: . . . . "the tragic in no opera sounds, or can sound, as it is heard in . . . . , or in the prelude in E flat minor of Bach's "Wohl-temperirte Clavier." Piano solo.

Answers to VIII.

A. Religious.

B. Unsatisfactory.

C. Tragically sad. Widow of a dead patriot.

D. Fanciful; full of fancy. Picture: Twilight; a woman playing and dreaming.

E. ? ? ? ? Non-significant.

F. ————————.

H. Sad.
I. Funereal.
J. Instability.
K. Not light enough for "*fantastic*"; too much matter for the merely negative "*disjointed.*" Whimsical.
L. Interesting, but to me not particularly beautiful or great. It seems incomplete, more like an introduction to something else.
M. Disjointed.
N. This kind of thing declines to be expressed except as, say, a seraph's song, a song of one excelling in knowledge.
O. Funeral march?

A. Satisfactory.
B. Soothing.
C. Heavily monotonous.
E. Known. Massive: the massiveness of a cathedral, with the delicate tracery of the frescoing and pillar ornamentation occasionally revealed by the light.
F. Gloomy.
G. Interesting and dignified; non-emotional.
H. Serious (philosophical), majestically elevated—but to a dizzy height à la Beethoven.
J. Languor; reluctance.
K. Contentment.
L. Feelings after a disappointment; not cheerless, but serious, and more uplifting than sad; at the same time more or less sad.

Question IX.

Can you connect the following melody with any marked type of personal character?

Mozart: Don Giovanni: Canzonetta in the second act, sung by Don Giovanni to a mandolin accompaniment, "Deh vieni alla finestra." It was assumed that there would be some, at least, in the audience who would not recognize the song. It seems to me to bring out the two fundamental characteristics of a Don Juan: his powers of passion and the mocking indifference that lies beneath. Gurney speaks (p. 469) of "the half gay, half tender gallantry of the Guitar Song in Don Giovanni." Attention was specially called to the contrast between the melody and the accompaniment. Violin and piano.

Answers to IX.

A. Known. Don Giovanni.
B. A conflict of emotions; on the whole the higher purpose triumphs over the baser.
C. Known; but it agrees with his character.
D. The violin melody, a child telling a story, or a story told to a child; the accompaniment suggests the lightness of the whole. It doesn't matter, after all.
E. D. J. and Laporello, nicht wahr? No other association, though I don't recall the last part.
F. Rollicking roué, the accompaniment; more depth of character in air.
H. Somewhat like the celebrated Don Juan Serenade; a passionate plea shut in in a laughing accompaniment.
I. Mountaineer; hunter. This was written before I noticed the air. Don Giovanni Serenade.
J. An earnest character with an underlying current of light-heartedness. One who thoroughly realizes the serious side of life, but who is

by nature an optimist and cannot entirely repress a natural joyousness.

K. David Rizzio. A combination of a Watteau shepherdess and Priscilla Mullins. Marie Bashkirtseff.

L. A person of merry temperament, but at the some time of serious intent. The running accompaniment gives this merry irresponsible character to it. The air on the violin is more serious, though at times quite merry itself, and also occasionally sentimental.

M. A fashionable or frivolous character with a deep undercurrent of sincerity. This idea did not come until attention had been called to the difference between accompaniment and violin melody.

N. Beyond the impression of the tempo, nothing expressible.

O. Isn't this Don Giovanni's Serenade? The feeling is in the air, the villainy in the accompaniment.

A. Levity; amorous sentiment.

C. Frivolity, and the earnestness really in the character.

E. Known. This does not suggest one particular type of character, unless, perhaps, it is the double character of frivolity with a steadying strain of seriousness.

F. The contradictory character of a young woman.

G. A complex, fascinating woman, amid gay surroundings; or, a grave and thoughtful woman wearing a smiling exterior.

H. Known. Don Giovanni.

K. Known. Don Giovanni. Flippancy, or better, playfulness, feigned, concealing a sad heart.

L. Known. Don Giovanni.

Question X.

Of what race should you think this song a product, and why?

"Der Rothe Sarafan." Russian popular melody. It has to my ear the monotonous sadness of the plains whence it has come. There is a terrible hopelessness in it; it seems an echo of immemorial misery borne with resignation.

Violin and piano.

Answers to X.

A. English.

B. The softness and the rather languid air about it betoken a southern nation, and I should say Persian.

C. Known; but I don't see why.

D. English.

E. Rothe Sarafan. Russian, I believe.

F. Irish; but too pathetic.

G. French.

H. German moonshine.

I. Old English; then recalled as Russian.

J. German. The music of a musical people, but of the peasantry because *simple*.

K. Central Germany.

L. Teutonic(?). Have no reason. Perhaps Slavonic(?).

M. English(?) or German.

N. Don't know it. Take it to be South German, but feel much doubt. Can feel no assurance.

O. Italian?

A. German — simplicity.

B. Not Italian, nor German, nor French.

C. A northern song, perhaps Norwegian or Russian, on account of its pathetic wildness.
E. German, because of its simplicity of feeling. Not light enough for France, nor sparkling enough for Italy, too much sentiment for England.
F. German.
G. Possibly Irish.
H. Known. Russian popular song. The melancholy is characteristic.
J. German. Sung by a homesick woman far away from her native land, possibly in America.
K. Unknown. Russian, because of its undercurrent of sadness.

Question XI.

Is the singer of the following melody a man or a woman, and out of what emotional experience would such a song be born?

Bizet. The song of Carmen in the third act of the opera, over the cards that foretell her death: "In van per evitar." The dramatic situation for which this song was written seems to me to be expressed in the music alone; a woman sings thus to tell herself that though life is sweet she must shortly die.

Answers to XI.

A. A woman has lost her husband in battle and is trying to console herself while singing to her young babe. Mingled despair and love for her child.
C. Woman; disappointed love.
D. Might be sung by either, possibly woman. Suggestion, the spirit of Schumann's "Ich grolle nicht."
E. First hearing. Either a man pathetically and simply pleading to a woman to have him, or a woman kindly and sweetly saying she must refuse. Second hearing. Decidedly a woman, and decidedly pleading, with a longing yet submissive spirit.
F. Woman; a farewell.
G. A man pleading with a woman who does not reciprocate his affection.
H. Maternal love.
I. Woman; jealous love.
J. Woman; intense protestation, sad, not angry.
K. Male.
L. Woman. Expression of some deep passion.
M. A girl mourning for her dead lover.
N. A woman resigns her dearest—a mother her son, a girl her lover,—for a noble end, a cause that demands him; she does not resign him to a blind fate, but to a cause. (Not at all recognized.)
O. A woman's sorrow and prayer. It is a sort of sublime calm despair.

A. A woman. A very loving plea for reconciliation, explaining her loyalty and faith in suffering.
B. Man; disappointment and despair.
C. Man; yearning wistfulness.
E. A woman who has suffered sings because conquered pain impels her.
F. Man; sorrow.
G. A woman; bewailing past joys.

H. Is it written for a man's voice? perhaps approaching death.
J. Man; passionate entreaty without avail.
K. A passionate woman; unrequited affection.
L. Woman; bewails loss of her lover; first wretched, then despairing, finally experiencing a gentler feeling and determined to make the best of it.

The evening began with a performance of the Andante from Mendelssohn's well-known and loved Concerto for violin and piano (Op. 64), on which three of the auditors took the following notes:

C. First movements of an undeclared love; middle of same; some disturbing circumstances in same, followed by repose.
K. Yearning; longing to go; attainment seen but recognized as impossible. Passionate pleading—struggle to transcend limitations.
The motive to the passion felt here are mixed. Partly they are the inevitable conditions of human life; partly the motive is love.
A. Education ethical; plea for purity and generosity; Strom der Welt; ethical practice on broad lines; justification; peace; beauty.

In ending the transcription of these notes of the first experimental attempt that as far as I know has ever been made to reach exacter notions in regard to the expressiveness of music, I cannot forbear acknowledging again my good fortune in obtaining aid of such temper and quality in the undertaking. Not only my thanks but those of all who are interested in the more careful study of the mental phenomena concerned in music hearing are due to this company of listeners for the zeal and the frankness displayed in these acute replies to an exacting series of questions.

Before proceeding to the examination of these suggestions from various music, let us endeavor to form some idea of the general nature of the inquiry in the pursuit of which they have been obtained.

To be continued.

PSYCHOLOGICAL LITERATURE.

PSYCHOLOGICAL NOTES.

By E. W. Scripture.

The Method of Regular Variation.

The method of minimum variation seeks to determine that change in the stimulus which produces a minimum change in the sensation. For this purpose the stimulus is varied in one of its properties till the variation is noticed. This is what the usual statement of the principle amounts to when we enlarge it from the exclusive application to the intensity of the stimulus. In applying this method to psychological questions it has up to the present day been changed in such a way that it can scarce be brought into agreement with the above view of it.

Given a stimulus possessing several properties x, y, z. Let one of these properties be varied, e. g. x, while the others are kept constant. We have thus two independent variables x and t, the varied property and the time. The problem is to find for what values of x and t the change becomes noticeable.

The expression for the variation of x is $\left(\frac{dx}{dt}\right)_{v = x_0}$. Now, if for $t = 0$,

and in general

$$\frac{dx}{dt} = u$$

where x_0 is a given constant (in this case the value of that property of the stimulus which is to be varied, or the "starting value"), and u is a given function of t, then x is determined for every value of t and the problem becomes: for a given value of the property x, which is varied at the rate u, to determine at what value of x the variation becomes perceptible. The value of x thus obtained is called the *least perceptible variation*.

The variation is likewise completely determined when for $t = 0$

$$x = x_0$$

$$\frac{dx}{dt} = u_0$$

and in general

$$\frac{d^2x}{dt^2} = U$$

where the quantities with the index $_0$ are given constants.

The quantity U as thus defined is called the acceleration of the variation. The value of x corresponding to the least perceptible change can be called *the least perceptible accelerated variation*.

In applying this method the property x is varied at the rate u, first increasing and then decreasing. This will give two values for the least perceptible variation, D_0 and D_u, from which we take

$$D = \frac{D_0 + D_u}{2} \text{ or } D = \sqrt{D_0 D_u}$$

The quantity D, however, is not a constant but a function of the rate of variation.

To find the least perceptible accelerated variation the intensity is varied, but at an accelerated (or retarded) rate. The results are treated in an analogous manner, and, denoting this quantity by A, we have

$$A = \frac{A_0 + A_u}{2} \text{ or } A = \sqrt{A_0 A_u}$$

A is a function of the acceleration U.

The starting value of x may, however, take on different values, x_0', x_0'', x_0''', ... Consequently the least perceptible variation and the least perceptible accelerated variation are not only functions of the rate of variation and acceleration but also of the starting value; thus,

$$D = f(u, x_0)$$

and

$$A = f(U, x_0).$$

Up to this point we have taken into consideration only one of the variable properties of the stimulus. In case it has more than one variable property, e. g., y, z, ... the least perceptible variation and acceleration of the property x may, through influences on the attention, etc., be dependent on the values of y, z, . . . If we distinguish the least perceptible variation and the least perceptible accelerated variation of the property x by the index x, then

$$D_x = f(u, x_0, y, z, \ldots)$$
$$A_x = f(U, x_0, y, z, \ldots)$$

Of course the least perceptible variations of y, z, ... are likewise

$$D_y = f(u, y_0, x, z, \ldots)$$
$$D_z = f(u, z_0, x, y, \ldots)$$
$$\cdot \quad \cdots\cdots\cdots$$
$$\cdot \quad \cdots\cdots\cdots$$

and the least perceptible accelerated variations are

$$A_y = f(U, y_0, x, z, \ldots)$$
$$A_z = f(U, z_0, x, y, \ldots)$$
$$\cdot \quad \cdots\cdots\cdots$$
$$\cdot \quad \cdots\cdots\cdots$$

If, with Wundt, we distinguish the minimum variation from the least perceptible variation, we get slightly different values for D_0, D_u, A_0 and A_u (see Wundt, Phys. Psy. I 350, Leipzig, 1887); in this case the term "minimum variation" and "minimum acceleration" are to be substituted for "least perceptible variation," etc.

In the applications of the method of minimum variation up to the present these quantities have been either neglected or treated in a manner not at all consistent with the method. The usual method or application can be illustrated as follows: Given the stimulus with the variable properties x, y, z, ... to find the least perceptible variation. One of the properties, x, is taken as variable, the others are made constants. Let the starting value be x_0. This starting value x_0 is given constant for an instant and then x is reduced to 0. This value of x,—that is, the absence of the stimulus,—is maintained for a time, n seconds; then another value of x is given, e. g. $x_0 + \Delta x_0$ and the person experimented upon is asked whether this value is perceived as different from the starting value of x. If not, x_0 is again given and after the same time, n sec-

onds, another value of x, e. g. $x_0 + \Delta x_0$, is given and the same question is again asked.

This is an entirely different problem from the one proposed for solution. Here we have the question of a judgment of the likeness or difference between two stimuli given at two different times, or between a stimulus $x_0 + \Delta x_0$ and the *memory* of a stimulus x_0 which occurred n seconds before. The fact that we are able to judge with great accuracy this likeness between a stimulus and the memory of a stimulus given a short time before is not to the point; although that problem is one of vital interest also, it is not the same as the problem of the least perceptible variation of the stimulus x.

I have said that the quantities considered above have been neglected in previous investigations. I must, however, make at least one exception, namely, the original and weighty essay of Hall and Motora, *Dermal Sensitiveness to Gradual Pressure Changes*, AM. JOURNAL OF PSYCHOLOGY, 1888 I 72, to a study of which I owe the impulse to a development of the method of regular variation and to its application instead of the old form of the method of minimum variation. This is, as far as my knowledge goes, the first time the method has been applied to psychological questions.

It is my intention to apply this method to a number of the problems of psychology. Two such investigations have already been begun, one on the faintest perceptible sound, the other one on the least perceptible variation of the pitch of tones; some of the qualitative results are given in the following notes. The apparatus in preparation will admit without any change the investigation of the problems of the least perceptible variation of intensity of pitch and of noises, of the lowest perceptible tones with certain (not all) degrees of intensity, and several others of which indications have shown themselves. A slight change will enable the solution of problems concerning the least perceptible accelerated variation; indeed, the difficulty of obtaining a constant rate of variation instead of an acceleration is what first called my attention to the subject of the perception of accelerated variation.

ON THE LEAST PERCEPTIBLE VARIATION OF PITCH.

Continuous variations of pitch are best obtained from the wave-siren. In the wave-siren a constant blast of air is directed against the edge of a rotating disc which is cut into waves so that a variation of the current of air is produced which follows the law

$$y = a \sin \mu t$$

where y is the change in the current at the moment t, a is the extreme value of y and μ is the vibration-index. This gives a simple tone, the pitch of which depends on the rate of rotation.

One of the chief purposes for which the wave-siren has been set up in Clark University is the investigation of the sensitiveness to variations of pitch according to the method of regular variation. The researches of of Delezenne,[1] Seebeck,[2] Preyer[3] and Luft[4] have been made with continually increasing accuracy, but all of them have left out of consideration the important element of time. The influence of this factor is taken into consideration and measured by employing the improved form of the method of minimum variation, namely, the method of regular variation. The arrangements for making the measurements are nearly complete; in the mean time the qualitative result has already shown itself.

1. DELEZENNE, Mémoires sur les valeurs numeriques des notes de la Gamme, Recueil des travaux de la soc. de Lille, 1826–27, p. 4.

2 SEEBECK, Beiträge zur Physiologie des Gehör- u. Gesichtsinnes; C. Ueber die Fähigkeit d. Gehörs, sehr kleine Unterschiede der Tonhöhe zu erkennen, Pogg. Ann., 1846 LXVIII 462.

3 PREYER, Grenzen der Tonwahrnehmung, Jena, 1876.

4 LUFT, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, Philos. Stud., 1888 IV 514.

As was to be expected, the least perceptible variation in the pitch of a simple tone is not only a function of the pitch and the intensity but also of the rate of variation. Given a tone of the pitch n and the intensity I where $I =$ constant, to find the just perceptible Δn when the pitch of the tone is varied at the rate $q = \frac{\Delta_n}{t}$. The preliminary experiments already prove that

$$D \propto \frac{1}{q}$$

That is to say, the least perceptible variation increases as the rate of variation decreases, and *vice versa*.

On the Faintest Perceptible Sound.

By means of a specially constructed audiometer I am able to vary the intensity of any given sound at almost any given rate. While preparing for an extensive series of measurements on the least perceptible sound I came across the following remarkable fact.

The audiometer is arranged so that the sound is heard in a telephone, and its intensity depends on the relative positions of the primary and secondary coils. At one point there is an absolute zero, that is, the plate of the telephone is not set in vibration. As the position of one of the coils is changed a vibration is set up in the plate, the amplitude of which depends on the strength of the current. If we change the position of the coil sufficiently the vibration of the plate and the air become great enough for the ear to perceive a sound. The point at which this happens — that is, the faintest perceptible sound — depends on the sensitiveness of the person tested and on several other factors, one of which has hitherto been left unnoticed, namely, the rate at which the threshhold is approached. The influence of the rate manifests itself in a peculiar manner. If the relative position of the coils is very slowly changed the faintest perceptible sound appears at a certain point where the physical vibration has a certain amplitude, which we can denote by p. Thus, when the amplitude of the vibration has been changed from 0 at the rate of q units per second, then the faintest sound perceived is p. But a change in the rate q, according to which the sound is varied from zero, produces a change in the faintest perceptible sound p, making it p'. That is, a change in the rate at which we pass from an absolute zero to the psychological zero causes a change in the relation between the two.

The apparatus for the absolute measurement of sound intensities in units of work has not yet been completed, so I can not give the exact law governing the relation of these quantities; but from the experiments as yet made one fact at least can be determined: the least perceptible sound has a greater physical value as the rate is increased. Judging from rough estimates I should say that this physical value varies as the square of the rate of change.

Notation for Intensity.

Although the modulations of intensity have been carried to a great degree of fineness in music, the amount of intensity in any given case has been left to the semi-conscious judgment of the musician and has not, like the questions of pitch and duration, been reduced to any regularly defined principles. In the history of music the first of the three dimensions of tones to push itself into conscious recognition was that of pitch; thereafter followed duration, and last intensity. The first complete notation for pitch is attributed to Guido Aretius in the XI. century. Three centuries later the notation for duration was introduced by Jean

de Meurs. Naturally the presence of exact means of expression for these two quantities afforded opportunity for development in the artistic execution on the one hand and for scientific research on the other. The subject of pitch has reached a high degree of development. The duration of tones is also a matter of technique that has been carried to a great degree of precision in practice, although it has been scarcely investigated scientifically. The intensity of tones, however, has been much neglected; it must be remembered that we are not speaking of the semi-conscious use of the different degrees of intensity in the execution or composition of a piece of music, but to a deliberate use of the shades of intensity. In music the consideration is confined to the five vague expressions, *ff*, *f*, *m*, *p*, *pp*. In science the question has been treated almost exclusively in relation to the rough tests for deafness. The late attempts to find a measure of the intensity of sound have come more from the efforts of physicists to satisfy the needs of the specialists for ear troubles than from any advance in the music of intensity.

Before much advance can be made, it will be needful to adopt some expressions for the different degrees of intensity to remedy the inconvenience and inaccuracy of the present terms. For the present purposes we can make use of a system limited to ten degrees of intensity. Pitch is indicated by the position of the note on the staff, duration is shown by the hooks on the stems of the notes, except in the case of the whole and half notes, where a difference is made in the head of the note. This change in the head of the note is unnecessary for the indication of duration and can be employed to indicate intensity. A very slight change is thus necessary in the present notation; we can retain the usual method of indicating pitch and the usual signs for duration with the exception of the two for the whole note and the half note. These can be indicated by two lines across the stem of the ordinary quarter note for the whole note and one for the half note. Consequently the series of notes as regards duration will be

representing the whole, half, quarter, eighth, sixteenth, thirty-secondth and sixty-fourth notes respectively.

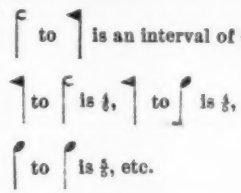
Whenever it is desired to write music without regard to intensity, it can be done in the same way as at present with the substitution of the two new signs for the whole and the half note, or it can be done as usual without any danger of there being a mistake in the playing of it. Moreover the comprehension and the execution of pieces in the usual style will not be in the least interfered with.

The heads of the notes in the above example are all the same; by employing different kinds of heads the different degrees of intensity can be indicated without introducing new complications in the notation. The degrees can be indicated by a series of new signs. Ten different degrees of intensity are to be represented; they can be said to stand in the relations expressed by the numbers placed under them in following list:

These can be used as the heads of notes directly on the staff.

The question arises of how to name intervals of intensity. To avoid the confusion, inaccuracy, and above all the limitedness of the names used for intervals of pitch, we can adopt a simple system in regard to intensity. An interval of intensity is to be designated by a fraction of

which the degree of the denominator is the degree of the first of the two tones, and the numerator that of the second. For example,



All of the degrees used at present can be indicated as well as several more. The worth of the notation does not, however, lie in this, but in the fact that these degrees and distinctions can be introduced into the notes on the staff, thus making it possible to manipulate the shades of intensity in music just as is done with pitch and duration.

A Constant Blast for Acoustical Purposes.

Töpler and Boltzmann in their essay, "*Ueber eine neue optische Methode, die Schwingungen tönender Luftsäulen zu analysiren*," Pogg. Ann., 1870 CXLI 341, make the complaint that it is impossible to obtain a constant pressure of air for blowing acoustical instruments, in spite of the regulators that are applied. They suggest the possibility of using some kind of rotary blower. In making some experiments with organ pipes a while ago, I was forcibly impressed with the same difficulty, although I had access to the best kind of bellows made, namely, that of Cavallé-Col as made by König. At a later date, when contemplating some experiments on the sensitiveness to variation in pitch, I came to the conclusion that unless a far greater degree of constancy in intensity could be obtained, any experiments on the sensitiveness to variation, either in pitch or in intensity, could not be made with the degree of accuracy necessary in psychological experiments. The first step was therefore to find a constant blast. After considerable time spent in fruitless trials I have adopted an arrangement which has proved quite successful.

The chief requisite is a rotary blower giving a sufficient pressure. The blower giving the greatest pressure with the least expenditure of power is the Root direct pressure blower. This, however, is inapplicable for acoustical purposes, owing to the vibrations of the air produced by the cams, which make a deafening noise when the blower is run rapidly. The difficulty is present to a far less degree in the Sturtevant blower. This blower employs a blast wheel of a special pattern, so arranged as to produce a movement of the air from the center to the periphery. Owing to a careful construction of the fans the pressure on blast thus produced has a constancy beyond any means of measurement at my disposal. Moreover, the adjustment of the center of gravity is so good that the wheel can be run at a high speed without danger. There are two disadvantages about the blower, namely, the high speed required to produce sufficient pressure and the production of tones in the blower when going at such a high speed. By courtesy of Prof. Michelson of the physical department of Clark University, I was allowed to run the belt of the blower directly from the drive-wheel of the engine, the drive-wheel being 31 inches in diameter and going at 340 revolutions per minute. The blower used is numbered 0000, stands 12 inches high, has an outlet of 2½ inches, and a pulley of 1¼ inch diameter. Driven by this engine the wheel makes 5600 revolutions per minute.

To get away from the noise of the machinery and the tones produced by the blower, a 1½ inch pipe is run to another room, where the sound cannot be heard. The sound brought through the pipe is so faint that it cannot be heard unless the ear is placed at the opening; it thus does not interfere with the use of the blast in any experiments yet tried. A specially made fan with curved blades is said to still further reduce the sound in some blowers lately made. The slightness of the sound produced by the blower is shown by the fact that in running the wave-siren from the lowest pitch up to the highest obtainable, no beats appear, whereas, an attempt to sing the tone of the siren near the rubber tube connecting the mouth-piece with the main pipe at once causes loud beats.

With the wheel running at the above rate and at a distance of 65 feet from the blower, a blast of 10 to 11 centimeters (4 in.) water is obtained. An engine such as here used is, of course, unnecessary. To supply a blast of 12 centimeters pressure with a discharge of 60 cubic feet per minute would require a speed of about 6000 revolutions per minute for the blower wheel and a motor of less than ¼ horse-power. If the motor has a large drive wheel the belt can be run directly from it and no counter-shaft is required.

The great importance of the arrangement here described lies in the greater degree of accuracy thus introduced into psychological experiments on hearing due to the possibility of maintaining a constant intensity.

Some Psychological Terms.

It is the purpose of the present remarks to propose short definitions for a few of the fundamental psychological terms. These definitions are not, however, to be considered as fixed; unchanging definitions are a sign of decrepitude. They ought to progress with the advance of the science; but at each stage of development it is absolutely necessary that certain names should be appropriated to certain things and that everybody should clearly understand just what they are appropriated to.

One of the most abused and indefinite words is "sensation." Bain uses the word to designate the mental impressions resulting from the action of external things on the body. Others take for granted a whole theory of the relation between the mind and the nervous system; Volkmann calls a sensation "the condition which is developed by the soul on the occasion of a nerve-stimulus that is brought to it." Sully defines it as an elementary mental phenomenon that cannot be defined in terms of anything more simple, its meaning being capable of indication only by a reference to the nervous processes on which it is known to depend; as almost absolutely nothing is known concerning these nervous processes, it would seem that the meaning of sensation cannot even be indicated. There is another class of definitions of which this from Carpenter can serve as a specimen: "Sensation is that primary change in the condition of the conscious *ego* which results from some change in the *non-ego* or external world." The easiest way is to shirk the duty of defining the word at all, as only too often occurs. From the lack of acquaintance of the principles of defining which is shown in those that undertake the task, the hesitancy of those who do not is readily understood.

And yet it ought not to be so difficult to agree on some definition. A term is a word used to represent a group of phenomena, a definition is a statement of the phenomena denoted by the term. To avoid the impossibility of going over all the phenomena denoted by the term, we can give a definition by stating what properties a phenomenon must have in order to be denoted by the term, what properties it may not have and

what properties are indifferent. Usually it is sufficient to give the essential properties, omitting the excluding and indifferent ones.

The crying necessity in psychology is a term that shall denote the simplest mental phenomenon, just as "atom" denotes the as yet undivided particles of matter. As such a term we propose the word feeling. Feelings would then be defined as the indivisible elements of mental phenomena; they are as yet unanalyzable components. Nothing is said of the relation to an external or an internal world; no hypothesis is introduced as the nature of mind; all that is assumed are the existence of certain phenomena, called mental phenomena, and the possibility of analyzing them. The ultimate results of the analysis may be what they will, we always have a name for them; with perfection of methods and apparatus the analysis will be pushed further so that what is to-day regarded as a feeling may prove to be a compound, nevertheless the term has always the same meaning.

The proposed definition is by no means entirely new; in fact the word most frequently used to denote simple mental phenomena of all kinds is probably this very term. It is used in almost this way by Spencer. "Each feeling . . . . . is any portion of consciousness which occupies a place sufficiently large to give it a perceivable individuality;" the definition proposed differs from his in not setting apart a special class of "relations between feelings."

Among feelings we find innumerable kinds. When we attempt to class them according to their likeness to one another, we find several ways of arranging them. The most usual way is to class them into two lots, according to whether we regard them as passively experienced or actively experienced. Here is just where we find the thought that is at the bottom of all the array of definitions of sensation; when stripped of theories we can well agree to appropriate "sensation" to the passively experienced feelings. In a like manner "impulse" can be used to designate the actively experienced feelings. These mental elements can, of course, be further classified into sensations of light, of sound, impulses to action, to inhibition, etc.

Terms for the indivisible elements having been obtained, it becomes necessary to have names for the compounds. To designate a psychological compound in general I venture to propose that much abused and at present indefinite word "idea." Possibly this may be justified by referring to an almost similar use of the word by Descartes (See Eucken, Grundbegriffe der Gegenwart, Leipzig, 1878, 225).

Ideas, however, seem to fall naturally into two great classes, according to the preponderance of sensations or impulses in their composition. The former class can be called "percepts" with scarce a departure from such a portion of definite meaning as the name now has. In a corresponding fashion the ideas composed mainly of impulses can be called "volitions." Compounds of higher grades can of course receive appropriate names.

The following list of terms is proposed for psychological use according to the definitions attached to them:

1. FEELINGS are the indivisible elements of which mental phenomena are composed. Every fact of consciousness that has not been proved to be a combination of other facts is to be called a feeling.
2. SENSATIONS are those feelings that are regarded as coming from without; they are passively experienced feelings.
3. IMPULSES are those feelings that are regarded as originated in the mind itself; they are actively experienced feelings.
4. IDEAS are compounds of feelings of any kind; they are the psychical units.
5. PERCEPTS are those ideas that are composed mainly of sensations.
6. VOLITIONS are those ideas that are composed mainly of impulses.

FACILITIES IN EXPERIMENTAL PSYCHOLOGY AT THE VARIOUS GERMAN UNIVERSITIES.

By William O. Krohn, Ph. D.

The writer returned in March from a sojourn of nine months on the continent. From two weeks to three months were spent at each of the principal German university centers. The time was occupied in a study of men, methods and laboratory equipment at those universities enjoying facilities for work in experimental psychology. Many teachers who contemplate starting laboratories in this country have suggested that we give information in the form of a printed article as to the apparatus used, its cost and value. We have already done this in several instances through the ordinary channel of private correspondence. The present article concerns apparatus only. We hope later to give a sort of pen picture of the men who are now contemporaries in furnishing the instruction in experimental psychology in the several universities of Europe which the writer has visited as well as to present the results of careful and rather extended study of their methods of work. The time was spent at Heidelberg, Strassburg, Zurich, Freiburg, Munich, Prag, Berlin, Leipzig, Halle, Göttingen and Bonn, in the order named. The longest periods were those at Freiburg and Berlin. At each of these universities the laboratories were carefully inspected and in some of them the writer carried on experimental work. In our description, for sake of convenience, we will follow the order given above. The description of the excellent laboratory at Göttingen, however, much to our regret, must be omitted from the present article.[1]

Heidelberg.

Heidelberg was visited in order to learn from Kuno Fischer and Caspari the real trend of psychology and philosophy at the present time, especially to ascertain to what extent the English Associationalists, and more particularly Herbert Spencer, had influenced German thought. Fischer is openly opposed to all experimental research in psychology. He regards it as a temporary fad, a side issue, an illegitimate method, lying wholly outside the main stream in the development of psychology as a science. Caspari is much more friendly, and reads lectures on experimental psychology, but does no laboratory work, and has had no laboratory training. To the medical faculty belongs the credit of the projected laboratory at Heidelberg. Prof. Kraepelin lectured on "Physiological Psychology" and succeeded in attracting and interesting enough men to warrant him in offering in a modest way a course of laboratory instruction. His equipment is small and he fails to distinguish between psychological and physiological experiments. Naturally the latter predominate.

Strassburg.

At Strassburg there is no psychological laboratory, but Goltz and his assistant, Ewald, have much to show that is full of interest. Goltz has a large collection of animals—dogs, monkeys, pigeons, salamanders, etc.—from which the cerebrum has been wholly or partially extirpated. Nowhere is vivisection carried on on a larger scale than at Strassburg. Indirectly there is much valuable material to be made use of at this university. Ewald is ingenious as a mechanical contriver. His newly

[1] The material collected for the portion of the article on the laboratory at Göttingen was loaned some weeks ago to a fellow-worker, who wished to order some of the excellent apparatus contained in this list, and has not been returned as yet. The writer makes apology to Professor Müller and Dr. Schumann, and promises to give their laboratory the full treatment it merits, in the next number of the Journal.

invented chronoscope has certain advantages over the Hipp machine and is much cheaper (95 marks). It is described at great length and illustrated by two cuts in a thesis by Otto Dumreicher.[1] The larger size is the more desirable. This little instrument is, in fact, only a counting apparatus which records the number of breaks in an electrical current which passes through the instrument. A tuning-fork that makes a hundred vibrations in a second is a very convenient and simple instrument for the breaking of the current into a number corresponding to the number of vibrations. Thus one obtains the time to be measured expressed in hundredths of a second. The chronoscope then merely counts the number of these breaks in the electrical current, i. e., the number of vibrations made by the tuning-fork. For the purpose of counting these breaks there is in the chronoscope a little electro-magnet which moves a little ratchet in and out. This ratchet or trigger in turn moves a little toothed wheel, which possesses a hundred teeth, and on this wheel is an indicator. In this way the movements of the electro-magnet which is attached to the ratchet are transmitted to the indicator so that a spring or weight is unnecessary. This is one of the most pleasing features of the apparatus, since no winding of clock-work is needed. It is so arranged that the toothed wheel can not be moved backwards, neither can a single tooth be skipped. The instrument can never make a false record. It remains only to speak of the way in which the indicator is brought back to zero. The ordinary complicated arrangement of a stop-watch would not serve the purpose of Prof. Ewald. He has solved the problem in the simplest possible manner. The entire inner part of the chronoscope (electro-magnet, toothed wheel, and indicator) turns upon an axis, while the dial plate over which the indicator moves is fixed and immovable. After each single test one can very readily turn the whole thing, indicator and all, back to zero on the dial plate. The key used by Ewald in connection with his chronoscope is equally ingenious. It consists of a double armed lever so arranged that the current is closed when one presses on the button, but opened as soon as one withdraws the finger. It costs 20 marks, and is illustrated in Dumreicher's paper as well as Majer's catalogue. Ewald's mechanic, Majer, also makes an excellent "inductorium," modelled after that of Du Bois-Reymond, but somewhat improved. It costs from 80 to 100 marks. His pseudoscope should be in every laboratory; it costs but 25 marks. Ewald has also invented a color mixer, which makes it possible to vary the sectors while in motion. It is not only very complicated and expensive (200 marks), but also very unsatisfactory as well. It doesn't always work and is frequently out of order.

Wendelband in his lectures pays some little attention to experimental psychology.

ZURICH.

Zurich attracts the psychologist mainly through Dr. Forel. His work is chiefly in the line of psychiatry. In his "Anstalt" one sees much interesting pathological brain matter — indeed the best at any of the German universities. Work in neurology and histology is carried on continually in a well appointed laboratory, which is under the immediate supervision of Dr. Forel. The writer is greatly indebted to this delightful man for showing him many hypnotic experiments. Hypnotism is constantly employed by Dr. Forel in this Anstalt as a means of cure. The clinics in psychiatry supply much interesting material for study and examination. Prof. Avenarius lectures on general psychology, but contributes nothing on the experimental side.

[1] Zur Messung der Reactionszeit; Strassburg, 1889.

FREIBURG.

The laboratory of Dr. Münsterberg is distinctively a *psychological* laboratory. A distinction is constantly made by him between psychological and physiological experiments. His laboratory is on a private foundation established by Prof. M. himself, out of his own resources. To use his own figure, "The university authorities appropriate scarcely enough to buy the little bit of quicksilver used." The laboratory has little more than a nominal connection with the university of Freiburg, and is located in two large rooms of his private house. The students work in the laboratory from 11 to 1 each day of the week, though it is open at all hours to such as wish to carry on research work in connection with special problems. His courses are "privatim" but "unentgeltlich." The laboratory is provided by the professor with all the current literature. His apparatus is all practical, designed by himself, and constructed by his mechanic, Elbs. Aside from the staple apparatus to be found in every laboratory (Hipp chronoscope, meteronomes, etc.), the following pieces are of especial interest and to be commended for their real value:

1. Muskelsinnapparat. This is the apparatus used by Delabarre in gathering material for his thesis which earned him his doctor's degree. In his paper he has an excellent cut of this apparatus.[1] This apparatus is made for the right or left arm. It is better to have two pieces of this apparatus so as to be able to make bi-lateral movements of the arms for comparison. The following is a brief description: Into a heavy iron stand moves a strong iron rod a little over an inch in thickness, which can be easily elevated or lowered at will, and by means of a screw can be held any height, something on the principle of a music stand or modern piano lamp. Above on this stand, arranged to turn about an axis, is the portion of the apparatus which is more immediately concerned in the experiment. This consists of two small rails, upon which runs a light easily moved car with four small brass wheels. In order to hold the car fast upon the track at any chosen position or angle, there extends out from the under side a piece of metal, which has upon the end a small wheel, that runs along the under side of a third rail. This third rail is midway between and a little above the other two. To the car is attached an indicator, the point of which indicates the position of the car upon a scale 900 mm. long. This scale is fastened upon the side of one of the outer rails and divided into half centimeters; millimeters can easily be measured with the eye. To the top of the car is attached a short hollow brass cylinder, into which the end-joint of the index finger can be placed in order to set the car in motion. Upon the middle track are two clamps, which in every chosen position can be fixed as limits for the movements of the car, if such limits are desired. At each end of the middle track are little pulley wheels, over which a string can be drawn, which is fastened at one end to the little car and at the other to a scale pan, which is used in experiments where the weight of the wagon is to be compensated or where the movement of the car by means of a weight is to be made more difficult or easier. The portion of the apparatus consisting of the rails and car is movable about an axis and can be made to stand at any angle between the horizontal and vertical position. Thus the car can be moved horizontally or vertically, or at any incline. By means of the compensating weights in the scale pan, the resistance of the moving car is almost entirely done away with. The apparatus is well made and lends itself to a multitude of conditions, and is exceedingly useful for many purposes. Price 130 marks.

2. A very convenient chronoscope, incased in wood so as to lessen the noise. It measures hundredths of a second very accurately and is

[1] Ueber Bewegungsempfindungen. Pub. by Epstein in Freiburg, i. B., 1891.

very useful in ordinary reaction-time work. It runs twenty minutes without re-winding. It is not electrical, but runs by springs. It is much more convenient than the Hipp chronoscope. It costs 200 marks.

3. Augenmassapparat. This consists of a wooden board about 600mm. long and 500 mm. high, standing upright on its side edge. It is covered with very dark green felt, since this color is less fatiguing to the eye. At the back of the board are two scales, one vertical, the other horizontal. These scales are sliding and divided into millimeters. By turning a screw, the scale, either vertical or horizontal, may be moved, and in moving the scale the two white points on the front of the board are moved nearer to each other or away from each other at will. The exact distance can be read in millimeters by referring to scale on the back. The subject is made to judge distance moved as compared to a certain norm or standard given at the beginning of each test. Thus the method of average error is the one employed. Münsterberg has made over 30,000 tests with this piece of apparatus.

4. A key-board for reaction-time determinations, consisting of 50 buttons in electrical connection. These buttons can be labeled to suit the experimenter. Price 19 marks.

5. Schall-apparat. This apparatus is constructed for the purpose of determining the direction of sound. It consists of a very small box containing electrical apparatus, that makes the ticking noise, which acts as auditory stimulus. This is enclosed in a large oak box, deadened by a lineing of felt and zinc, and the space between the two boxes filled with water. The electrical apparatus can be set going from without by means of a small Grove cell battery. Through one side of the outer box are inserted 3 iron tubes that enter the inner box, and through which the sound can be transmitted. To these are attached rubber tubes about four feet long, at the end of which are bell-shaped guttapercha cups that fit over the ear; these can be held at any angle, and thus the sound made to come from any direction. The subject then makes his judgment as to the direction in each case. It is a very convenient and ingenious device, and cannot be gotten out of order. Price 60 marks.

6. Fall-apparat, costing 20 marks.

7. Sphygmograph, 250 marks.

8. Color-mixers which are run by electricity are also to be recommended for their simplicity of construction and their general satisfactoriness. The laboratory also contains a "Ton-messer" of large compass constructed by Appum of Hannau, a / m.

A word ought be said with reference to Münsterberg's method of instruction. He sets apart each day for some specific class of experiments. Thus, Monday might be the "color-day," in which the students have the advantage of seeing all the apparatus connected with color-work. Tuesday would be the "sound-day," in which the experiments concerned sound alone. Wednesday would perhaps be devoted to psychometric methods, and so on through the week. After several weeks of this sort of introductory work, the students who are inclined select special problems for research, and others assist the professor in carrying on his own investigations. During the past year the professor has made many thousands of experiments that bear upon the general subject of daily rhythm. Some of this material will be treated in his paper at the congress to convene in London this summer. Professor Flournoy, in establishing his laboratory at Geneva, ordered duplicates of all of Münsterberg's apparatus.

Mention must also be made of Prof. von Kries, who is located at Freiburg with one of the best equipped physiological laboratories in the world. It is in a large, brilliantly appointed building, recently constructed, and well supported in a financial way by the government of

Baden. The lectures by this professor on the "Physiologie der Bewegung u. Empfindung" are replete with interest for the psychological student as well as are some of his practice courses in the laboratory under his immediate direction.

MUNICH.

At Munich there is no university laboratory in experimental psychology. But this does not imply that the students in this university are entirely without facilities in this line, for Professor C. Stumpf has a very useful though unpretentious collection of apparatus for sound experiments accumulated by him in order to carry on that large amount of experimental work in this field, in which he is recognized as an authority, and in which he has labored so assiduously. His methods are of the most painstaking sort, and "exactness" is a fitting label for all his experimental work. He works the same problem over and over again regardless of time involved, subjects all his results to a revision in which the most exacting tests are used, confirming and reconfirming his previous conclusions, and all this time aware that the world is awaiting his long promised third volume on "Ton-psychologie." He has an especially constructed "Ton-messer" (Appum), and the best series of forks found anywhere, which were taken from a piano constructed entirely of tuning forks. He uses much other apparatus along the other lines of psychology to illustrate his lectures in the class room.

The writer is indebted to Dr. Freiherr von Schrenck-Notzing, who showed him much in the way of hypnotic experiments. This well known physician constantly makes use of hypnotism as a therapeutic agent. He is also a close student of psychology, as Stumpf, Edward von Hartmann and others testify. Schmidkunz, a young man, lectures at the university on hypnotism once a week. Stumpf's lectures on applied psychology or pedagogics constitute an interesting and valuable course.

PRAG.

In the German university at Prag, Prof. Marty has a small collection of apparatus for purpose of illustration. It is on a very small scale, and no research work is done in psychology. Professor Hering's work in connection with the physiology of the end-organs of sense makes his laboratory an interesting place for the psychologist. The laboratory or institute consists of ten very large and well appointed rooms; the professor has three assistants. His mechanic, Herr Rothe, will furnish catalogue of instruments on application. Some of the apparatus for color work is absolutely indispensable to the well equipped psychological laboratory.

It might also be mentioned in passing that Herr Fric is located at Prag, from whom excellent models, charts and prepared tissue can be secured.

BERLIN.

Professor Ebbinghaus is at the head of the work in experimental psychology at the Berlin University. The authorities have set aside two rooms for his use. He has but little apparatus—Hipp chronoscope, tuning forks, brain models, meteronomes, and in general such pieces as are found in every laboratory. Special mention must be made of a color-mixer so designed that one can change the sectors without stopping the machine. It is very ingeniously conceived, but poorly made by the mechanic. Professor Ebbinghaus is exceedingly clear in his lectures, which are well attended, and he deserves from the powers that be, better support in a financial way to carry on the work in experimental

psychology, work which he has well begun and for which he is so admirably fitted. It is greatly to be regretted that Germany's largest university is not better equipped with apparatus.

Munk's work at Berlin is very similar to that of Goltz at Strassburg. His lectures should be attended by all students in psychology who have the opportunity of studying at Berlin. His work in extirpation is always of profound interest. One of his students, Dr. Max Dessoir, who has recently distinguished himself by an elaborate series of interesting and important experiments upon the sensations of the skin, has just been made docent in the university. He proposes to extend the study to all the other senses as rapidly as he is able to do so. His work in connection with the temperature sense was carefully observed by the writer of this paper, with interest and profit. This young investigator has through his recent publications put some new and valuable material into the hands of psychologists. His lectures are upon "Psychological Basis of Æsthetics." The student at Berlin has access to Goldscheider, Preyer, Du Bois-Reymond and Helmholtz, besides the advantage of hearing Zeller, Lazarus, Dilthy, Döring and Paulsen. There is in Berlin a society composed of men interested in experimental psychology, which holds meetings every two weeks, at which a paper embodying the results of original work is read by some member or invited guest. These papers are afterwards published in the society's "Transactions." Dr. Dessoir is secretary of the Berlin society, and Dr. von Schrenck-Notzing of a similar one at Munich. They partake in some degree of the nature of the American Society for Psychical Research. Dr. Moll, the hypnotist, who is no less a psychologist, is a member of the Berlin society.

Leipzig.

Professor Wundt's laboratory is so well known, and his apparatus has found way into so many places, that perhaps it needs no detailed description, and yet the writer feels justified in giving a complete list in the hope that it will be of some assistance to those who are projecting laboratories — the persons for whom this article is chiefly written. Some of the apparatus is a little antiquated and has been superseded. Still, even this is useful for the purpose of illustration and verification. Professor Wundt has two able assistants in Dr. Külpe and Dr. Kirschmann. The laboratory or "institute," as it is officially styled, embraces six rooms conveniently arranged and tolerably well lighted, but with poor floors. Wundt contemplates moving to other quarters. The apparatus consists of one new Hipp chronoscope, price 282 marks. One Kugelfallapparat to test chronoscope; this is of little value and now seldom used, 64 marks. One large control hammer, which occupies 7 tenths of a second in falling; this is for the purpose of regulating the chronoscope, is finely constructed and costs 275 marks. It is accurately described in the last number of the Phil. Studien (Vol. VIII., page 145 ff.) by Külpe and Kirschmann. Cuts of the instrument are shown in connection with this article. One rheochord, 15 marks; one Fallapparat with a slit in the plate displaying the letter or word, 125 marks; one Fall-chronometer with automatic contact (Cattell), 145 marks. This is described in the Phil. Studien, Vol. III., pp. 307 ff., and is a very useful piece of apparatus. One "Sprech contact-apparat," in which the current is made or broken by speaking into a membrane-covered drum, costs 33 marks; one Schall hammer with electrical connections, so that it strikes when current is made or broken; meteronomes with and without bells, 12 and 15 marks respectively; one adjustable electro-magnet with stative, 9 marks; color mixers, run by clock-work, 54 marks each; the same with governors attached, 64 marks; one electric chronographic tuning fork of 250 vibrations, 90 marks; the same

(Helmholtz) with 125 vibrations, 75 marks; tuning forks mounted on resonant boxes, cost according to size; one reaction apparatus of ten buttons in electrical connection (Merkel), 56 marks. One large, heavy pendulum with horizontal and vertical slits in the bob. The subject, looking through tube as the pendulum passes, discerns the letter or word back of the pendulum, as it is displayed through the slits when the pendulum oscillates to and fro. Two adjustable magnets, reaction-time studies of light impressions, 275 marks. One chronograph for the measuring of very short time-intervals, consisting of large drum revolved rapidly by means of heavy weight. There is a cut of this apparatus in Wundt's Phil. Studien, Vol. IV., p. 458. It is extremely expensive, costing 700 marks; the same results can be obtained by a revolving drum propelled by a little three-dollar electric motor. One Zeitsinnapparat with six contact keys. This consists of a vertical drum revolving within a circle on which is a graduated scale. To this circle can be attached six contacts at different distances, the distances read in degrees, minutes and seconds by referring to the scale. A point on the drum in revolving touches these separate contacts in turn, thus making a current at each contact, which is communicated to an electric bell or hammer. The subject then judges and compares the time intervals between the sounds; costs without drum 124 marks. It runs by weights. It is described by Ester in the "Studien," page 38 of volume II. With the large drum and recording apparatus it costs 350 marks. One large Fallapparat with 4 electro-magnetic ball-holders. This is the piece of apparatus used by Professor Angell of Stanford University when at Leipzig in determining, according to the method of average error, the estimation of various intensities of sound. The apparatus consists of 4 upright polished iron or nickel rods, on each one of which is a little holder for the small ball. In making the current these holders drop the balls upon a block of ebony. As the balls are elevated more and more the sound becomes more intense. With four of these upright standards four successive sounds of different intensities can be obtained without re-adjustment. This piece of apparatus is more fully described in the "Studien," Vol. VII., p. 425. One 3-fold diaphragm with an arrangement whereby the square holes can be readily adjusted, by means of a micrometer screw, to various sizes. Through these holes one looks at sheets of colored gelatine paper. The object of this apparatus is to experiment upon the relation of color to the apparent size of the surface. The apparatus was designed by Dr. Kirschmann, and is described and illustrated by him in Vol. VI. of the "Studien," page 432. One large chronoscope used for demonstration purposes in the class room; it has a large ground glass dial, 40 centimeters in diameter and lighted from behind like the dial of a tower clock. Costs 400 marks. An apparatus for the demonstration of after images costs 48 marks. A similar apparatus for demonstration purposes by means of which one compares the endurance of the after images of sound and light, 60 marks. One model illustrating the movements of the eye, 45 marks. One model demonstrating the retinal images, 34 marks. One large "rotation-apparat," a sort of colossal lecture room color mixer, costs with colored disks 175 marks. To this list must be added various keys, contacts, commutators, which are as a rule much better made in America. Any or all of the above apparatus can be secured from Wundt's mechanic, Herr Krille.

One of the most important pieces of apparatus, because of its utility, is the Baltze kymograph. It is absolutely indispensable—more so than the chronoscope—to a laboratory in experimental psychology. The late one is much improved over previous ones, and is shown pretty well in a cut on page 19 of Langendorff's new book, "Physiologische Graphik."

Only two psychological laboratories in all Germany were equipped

with this admirable piece of apparatus, the one at Leipzig and the one at Bonn. The following is a brief description:

A carefully turned hollow brass cylinder is set in rotation by means of a finely constructed piece of clock-work, which is contained in a brass box. This clock-work is regulated in its going by a pair of governing fans, after the principle of Foucault. A small lever serves to start and stop the clock-work. The clock-work turns a horizontal metal axis, to the end of which is attached a metal disk (about five inches in diameter), and which in rotating turns by means of friction the upright axis of the drum or cylinder already mentioned. By a simple and sure device the speed of the rotating drum can be accurately regulated. The drum can be made to turn in a horizontal as well as vertical position, and also has a little attachment whereby use can be made of a little electric motor or a heavy weight, if very great rapidity is desired. The drum of the new Baltze apparatus has a height of 13 centimeters, and is 50 centimeters in circumference. This drum is covered with a strip of smoked paper, on which are traced the curves. By a series of exchange wheels, in different combinations, the kymograph can be made to rotate the drum anywhere between 3 seconds and 90 minutes. By using a weight and tying down the governors (which can be done without injury), Tigerstedt succeeded in turning the drum at such a rate that every millimeter on the paper corresponded to .0016 seconds in time. The student in psychology at Leipzig has also the advantage of Ludwig's renowned "Physiological Institute." Baltze, who contrived the kymograph which we have attempted to describe, is Ludwig's mechanic. Wundt delivers lectures on history of philosophy, but no lectures on psychology, leaving the latter task to his assistant, Külpe, whose long laboratory training gives him a fund of material for illustrations along every line of experimental work. Glockner's lectures on pedagogical psychology, as well as those of Leydl, are exceedingly interesting and valuable. Ludwig's lectures on "Empfindung and Bewegung" are more *psychological* than physiological, and should be down on the schedule of every student who pretends to study psychology at Leipzig.

Halle.

At Halle there is no laboratory at present in experimental psychology, though one is contemplated. The writer visited Halle to confer with Professor Uphues and Dr. Huffert, as well as to hear Benno Erdmann in his lectures.

Jena.

It was impracticable to include Jena in our "Rundreise," much to our regret. However, Dr. Ziehen of the medical faculty, lectures on physiological psychology, and his courses are very popular. His lectures have just been published in outline, and a helpful and suggestive book they make. He also lectures on brain-anatomy (privatim), and on psychiatry (publicee), both one hour a week courses. Professor Biederman in the practice courses in his "Physiological Institute" offers much of interest. He is, as his record as a lecturer and writer shows, most deeply interested in psychology in its modern garb.

Bonn.

The psychological laboratory at this university has an interesting history indeed. Toward the end of the year 1887, Professor Lipps, now in Breslau, and Dr. Martius, the psychologist at Bonn, proposed to the philosophical faculty and university senate that appropriate rooms be set aside for the purposes of experimental psychology. Through a

lecture on the "Aims and Results of Experimental Psychology,"[1] this endeavor became widely known. The existence of the present laboratory was, however, not the result of these overtures, which were rejected at the time.

The rooms in which the psychological investigations are at present made belong to the Physical Institute. They were generously placed at disposal by Professor H. Hertz, the famous electrician and successor to Professor Clausius. The psychological laboratory at Bonn must, for the present, be looked upon as a private undertaking; it does not enjoy the patronage of the state. The admission of the students occurs in the form of a "privatissimum."

The arrangements are in general copied after those of the Leipzig laboratory, nearly all the apparatus being duplicate of Wundt's own. The most important instruments are all at hand. One of the late acquisitions is a Baltze kymograph of the most excellent workmanship. There is also an improved form of the Angell apparatus for investigations along the line of sound-intensity. The rooms, four in number, are admirably adapted to the purpose for which they are used. Indeed there is no psychological laboratory in all Germany equally fortunate in this respect. The dark room is a little gem, and a model of its kind. For the rest we shall let Dr. Martius speak for himself, and append a quotation from an interesting letter, which was, however, not written primarily for publication.

"The participation of the students in the investigations is naturally small. There is lack of such as are inclined to devote themselves for a long time constantly to psychological investigations. Workers are always present, but the facilities exceed the demand.

"This brings up the question: To what extent should the students take up independent work in the psychological laboratory? The experimental method in psychology has achieved such brilliant success that a scientific treatment of psychology which is not based on this foundation has become at the present day inconceivable. The fundamental facts of mental life, the sensations and perceptions, can be investigated only in this way. Moreover, the truth that a thorough knowledge of the facts of consciousness is the proper foundation for the remaining philosophical disciplines is daily receiving wider recognition. Thus the progress of psychology and philosophy depend to no small degree upon the development of experimental psychology. Wundt once made the remark that he expected to live to see every university in Germany provided with a psychological laboratory. If a retrograde movement does not occur, this expectation will surely be soon realized. The state authorities in the very interests of their universities will not be able to withdraw from the responsibility of furnishing a solution of this problem.

"This, however, does not decide how far the independent psychological investigations in the laboratory are to be open to the student. It appears to the writer not to be in the interest of psychology itself for this to occur to too great an extent. What every student who is to be examined in psychology (and also every student of medicine) ought to know, can be presented in lectures, provided the proper apparatus and lecture-rooms are at hand. The psychological work itself, on account of its difficulty, should be reserved for a limited number, who have a special interest in and enthusiasm for the subject. The future academic instructor in psychology should have a thorough preliminary psychological training if he is to be anything more than a historian and interpreter of psychological systems. For the purposes of instruction, however, lectures well supported by demonstrations are sufficient. It

[1] "Ueber die Ziele u. Ergebnisse der Exper. Psychologie." Bonn, 1888; 5 pp.

is, moreover, to be expected that with such limited and modest demands the authorities will be more inclined to keep pace with the progress of the times."

Dr. Martius lectures twice a week (privatim) on Grundzüge der Psychologie (with demonstrations), and directs research work one hour each day (privatim and gratis). Professor Neuhauser lectures on general psychology four times a week. Professor Nussbaum of the medical faculty lectures on the "Anatomy of the Sense-organs," Dr. Koch on the "Physiology of the Sense-organs," the former two hours, and the latter once a week. The latter also lectures on hypnotism, sleep and narcosis. Professor Schaefhausen lectures on anthropology twice per week. Pflüger's "Seminar" is given over largely to subjects of great interest to the psychological as well as the physiological student. Meyer lectures four times a week in a charming manner upon history of pedagogy.

This concludes our description, which is of necessity "sketchy," and in a measure incomplete as to details. We shall take pleasure in answering inquiries of those wishing more detailed information.

UNCONSCIOUS SUGGESTION.

During the summer semester of 1891, I gave a course of lectures on hypnotism in the auditorium of the Burgholzli Asylum (Zürich) to the students of the medical faculty of the university. At one of the lectures the young and intelligent attendant K. from Würtemberg was used for purposes of demonstration. After a few suggestions he fell at once into a deep sleep. I then gave him various post-hypnotic suggestions of a hallucinatory character, which succeeded well. In a water-bottle he saw several gold-fish that were not there; he saw a suggested cat, felt of and stroked her, etc. I then suggested to him in hypnotic sleep that on waking he should feel a strong desire for defæcation and that he should ask me for permission to leave the room immediately. He was scarcely awake before he complained to me in a low voice and asked to be allowed to leave the room. I allowed him to go and thought no more about him. A few hours later the assistant physician told me that the attendant K. had been attacked with violent diarrhœa and vomiting, together with headache. He had attempted to suggest these symptoms away, but in vain. I had the attendant called to me, as it was evident that my suggestion had had a much stronger effect than I had intended—a thing that as good as never occurs with me. After K. had recounted the symptoms of his suggested cholera I hypnotized him again and declared briefly and decisively that all his symptoms were past and that in the future he would never again experience anything more than would be contained in my suggestions. On awaking he declared that he was perfectly well and departed. On the next day I asked him how he felt and he replied that he was perfectly well, with the exception of a slight headache. This astonished me, as I knew that headaches are very easily suggested away. I asked concerning the nature of these headaches and he answered me in these words: "I have had these headaches for two years (he had been only a few weeks in the asylum as attendant). At that time I had an inflammation of the lungs (pneumonia) and with it severe headaches. The physician said to me that these headaches would never leave me again, as they were an inheritance from my father. In fact I have never completely lost them since then. Sometimes they are more severe, sometimes weaker, but even when I am in the best condition I always have a dull feeling of pressure in the head."

This declaration of the young man opened my eyes. That he was very suggestible had been proven by the intense effect of the previous suggestion. It now seemed to me highly probable that his two year headache was nothing else than the result of an unconscious suggestion

on the part of the physician who had treated him for his pneumonia. I questioned him in detail. He said that his father had suffered his life-long with headaches, but that he himself had in his youth only temporary headaches. Only since the pneumonia, i. e., since the remark of the physician, had the headaches increased, and becoming completely localized, had never left him for a moment. I then declared to him categorically that the physician had at that time made a stupid remark, that the headaches were not an inheritance and could be easily removed and that I would now cure him completely. I hypnotized him again, laid my hands on both sides of his head, suggested a strong electric current (which was of course not present) and declared the headaches to be now completely and definitely past forever. When he awaked the headache was gone. And in fact this single hypnotizing was sufficient to remove the headaches, at least up to the present;—to be sure, only four months have passed. After exhaustive night-watches and the like he has a few times had temporary headaches, but the chronic trouble is over.

This case seems to me very instructive, because it shows how easily physicians without knowing it can produce sicknesses that are not present by means of pessimistic prophecies, by anxious looks or by making the patient anxious. These are diseases suggested by the physician; they are not imaginations of the patient, but can be really painful, obstinate and serious troubles. Just as suggestion can cure an existing sickness, so it can produce one that does not exist. It is a two-edged sword, but is dangerous only when used by people that do not understand it. For this very reason it is necessary that the subject should form part of a physician's training.

In earlier years I myself evidently committed the very same fault as the physician just mentioned. An attendant suffered, as on previous occasions, from disturbances of the digestions and said she had pains in the stomach. We made an investigation, palpated the stomach carefully, asked if she felt pains, to which she answered in the affirmative. We thought we had found the sore place, looked sober and ordered milk diet and strict rest in bed, for we thought we had found an ulceration of the stomach. The cardialgia and the painfulness for pressure on the particular place in question grew continually worse; the patient could scarcely retain the milk, became emaciated and lamented much. Nevertheless no hemorrhage followed and otherwise no objective symptom could be discovered. For several months we kept her in bed. Gradually she recovered; yet for years the stomach remained very sensitive and we really believed that an ulceration of the stomach had taken place. After I had at a later date, 1887, through Bernheim in Nancy, become acquainted with suggestion, I hypnotized this attendant also. She proved to be extremely suggestive; everything succeeded with her, even the most absurd and intensive effects. It was a light matter to produce complete health in her case. Since that time we have had no occasion to trouble ourselves about her health. In the two other institutions in which she has since been employed it has remained excellent. In my mind there is not the slightest doubt that her former long sickness, together with the emaciation and debility, was called forth by our anxious investigation and the strict regulations made through fear of a perforation of the stomach. She surely had no ulceration of the stomach, and if we had at that time been acquainted with suggestion we could have cured her digestive troubles, together with the cardialgia and pains from pressure, in a few days.

PROF. A. FOREL.

EYE TESTS ON CHILDREN.

Tests on the strength of sight were made in the public schools of Worcester, Mass., U. S. A., in connection with the physical measure-

ments carried on last spring. In tabulating the results those cards showing defects of less than fifteen per cent. from the normal were thrown out, as it had been found that an almost imperceptible decrease in the amount of light had a comparatively great effect on the result and consequently these cards were within the limits of error. A cloudy day likewise caused the percentage to drop, as did also fatigue. In sorting the cards after this elimination, it was found that out of 793 boys 308 or 38.84%, and out of 602 girls 313 or 52% were short-sighted.

The cards were now arranged according to the school-grades. In the first two grades the percentage of defective eyes is lower for girls than for boys, but in the others the reverse is the case. On the other hand the results vary from grade to grade. Both boys and girls start with a low per cent., 35 for the boys and 31.4 for the girls. In the next two grades a great increase is observed, the figures being 52.7% and 67% for boys and girls respectively. In the fourth grade there is a drop to 38% and 48.9% respectively, in the fifth a slight increase to 41.6% and 51% and then a steady decline to the ninth grade, where it is 18% for the boys and 24% for the girls.

The importance of this fact lies in its bearing on the question of the influence of growth on the susceptibility to disease. This rapid decrease in the percentage of defective eyes corresponds in time with the acceleration of growth attendant on the period of adolescence. It has generally been supposed that this increased rapidity of growth is attended by an increased susceptibility to disease and injury, but the observations made by Dr. Axel Key in the Swedish schools seem to completely refute this idea. He found that at no time were children better able to withstand disease than at this period, while before and after it they were especially susceptible. The decrease in the percentage of defective eyes at this period may be accounted for on the same ground.

The cards were also sorted with respect to the amount of weakness of sight found in each sex and finally with respect to the amount of weakness displayed by each of the eyes. Among the boys 46.5% were between 0.62 and 0.85 of the normal strength, 24.2% were between 0.50 and 0.62 of the normal, while 30.25% were below this. Among the girls the percentages were 47.8%, 24.25% and 28% respectively. Among the boys both eyes were defective in 53.77%, the right eye in 19.6% and the left eye in 26.63% of the cases. For the girls the figures were 56.58% for both eyes, 20.55% for the right eye and 22.86% for the left eye. From this it would seem that the left eye in both sexes is more likely to be defective than the right eye and in boys more often than in girls. Among boys the defects seem to be more serious in a larger number of cases.

The instruments used in the tests were the Snellen test-types for the first five grades and the Galton eye-test for the other grades. The use of the former was necessary in the lower grades on account of the youth of the children. The variation between the results of the two instruments falls within the limit of error for fatigue or change of illumination and so has no influence on the results as reckoned above.

G. M. WEST, PH. D.

REACTION.

KÜLPE UND KIRSCHMANN. *Ein neuer Apparat zur Controle zeitmessender Instrumente.* Beschrieben von O. Külpe und A. Kirschmann. Phil. Stud., 1892, VIII. 145-172.

TITCHENER. *Zur Chronometrie des Erkennungsactes.* Phil. Stud., 1892., VIII., 138-144.

Dr. Külpe and Dr. Kirschmann describe an instrument devised by Prof. Wundt to regulate the Hipp electric chronoscope. It is now

recognized by psychologists (though apparently not yet discovered by physicists) that the chronoscope only gives correct times when the relative strength of the current and spring are so adjusted that the latent times in drawing up and releasing the armature are alike. The chronoscope is empirically regulated by measuring a standard interval of time, and adjusting the current and spring until the chronoscope gives this time correctly. Prof. Wundt has constructed a large falling hammer with a lever and weight, which give times up to 616σ. The time required for the hammer to fall cannot be calculated theoretically and can scarcely be measured with a tuning fork, but may be determined with a chronograph. According to the tests made by Külpe and Kirschmann the mean variation of the hammer as tested by the chronograph was on the average 1.04σ. This includes the variable error of the chronoscope, but not the constant error which would be carried over to the chronoscope. The variable error of the chronoscope and hammer combined was on the average 1.04σ, which may be regarded as exactly the same as before. The variable errors of the chronograph used at Leipzig and of the chronoscope are consequently alike, which is contrary to Wundt's statement: "Die Feinheit und Genauigkeit ist also hier (with the chronograph) eine reichlich zehmal so grosse als bei dem Hipp schen chronoskop." (Phys. Psy. II., 282.) Külpe and Kirschmann find the error of the chronoscope to be much greater when the hammer is placed in a secondary circuit. A secondary circuit is necessary in measuring reactions with the old form of the chronoscope, and this should consequently be discarded. The constant error of the chronoscope used at Leipzig was found to be over two and a half per cent of the time, and this correction should probably be made in researches from the Leipzig laboratory, in which this chronoscope has been used. The chronoscope may, however, be readily regulated so as to have an error less than one-fourth of one per cent. Külpe and Kirschmann find that the time the current is broken does not appreciably affect the latent time of magnetism, consequently if the chronoscope be regulated for a standard time, say 100σ, longer times will also be measured correctly. The old form of the chronoscope (in which the hands run while the circuit is broken) was used in these experiments, and they should be repeated with the newer and more convenient form, in which the hands may be made to run while the circuit is closed.

Mr. Titchener gives the results of experiments in which the distinction between muscular and sensory reactions was used to determine the time of perception. The paper is the first part of a research concerned with the time of association. It is maintained by Wundt that if in reacting an observer in one case direct his attention to the movement and in another to the stimulus, the difference in time will give the time required to perceive the stimulus. On this supposition Titchener obtained the following times (in thousandths of a second) for three observers.

Excess in time of sensory over muscular reactions.		
81.4	84.4	97.
Perception-time for a color.		
29.5	30.2	28.1
Perception-time for a letter.		
53.5	52.7	51.5
Perception-time for a short word.		
51.8	50.1	45.3

J. McK. CATTELL.

Columbia College.

June 6, 1892.

DR. G. S. HALL:

DEAR SIR:—I will gladly do my best to give you some account of the philosophical work at this university. The time, however, is not very propitious, for we stand in Scotland just now between the old and the new. The Universities' Commission, now sitting, has just framed ordinances (which come into operation next session), under which greater latitude is allowed to the ordinary student, i. e., the way of choosing his curriculum, and at the same time greater opportunity is offered for specializing in honours courses. We hope that this may lead to a considerable increase in the number of honours students, and a corresponding advance in the amount and the standard of higher teaching in the different departments. It will be necessary to this end that the university staff of teachers be increased; and from the increased funds at the disposal of the Commissioners we look for an improvement in this respect also. In philosophy, for example, the Faculty of Arts here has asked for a lecturer in physiological experimental psychology, and a lecturer on ancient philosophy.

These things, however, are still in the future, and I can only speak of the past. There are only two official teachers of philosophy in the university, the professor of logic and metaphysics and the professor of moral philosophy; though we have also a professor of political economy, a professor of the theory, history and art of education, and a professor of public law, all included in the philosophical department of the future. The lectures of the professor of education (Professor Laurie) at present embraces a large amount of psychological training, and the theological lectures of Professor Flint often diverge into philosophy; and these well-known names could not be omitted in any estimate of the philosophical work of the university. Philosophical teaching in the stricter sense, however, devolves entirely, as I have said, upon Professor Calderwood and myself. Every Scottish M. A. has hitherto been obliged to include "logic and metaphysics" and "moral philosophy" in his curriculum, and hence the energies of a Scottish professor have to be mainly devoted to the elementary training of the large masses of students which, in a university like this, pass annually through his hands. This leaves less time than is desirable for the development of honours teaching.

The ordinary course in "logic and metaphysics" embraces the elements of logic and empirical psychology, together with an introduction to the problems of epistemology and metaphysics, such as may be gained from a study of English philosophy in Locke, Berkeley and Hume. The character of Professor Calderwood's course in moral philosophy may be best gathered from his much-used "Handbook" of the subject. In connection with both chairs there has existed, for many years now, an advanced class for honours students. In "logic and metaphysics" this has been devoted hitherto entirely to metaphysics and the theory of knowledge in connection with the history of philosophic thought, especially of the Kantian and post-Kantian era. The corresponding class in "moral philosophy" embraces a survey, historical and critical, of ancient and modern ethics. Finally, it would be wrong not to mention that to each of the chairs is assigned a class assistant, who, in addition to assisting with the paper-drudgery of examinations and essays, gives a certain number of lectures, both to the ordinary and the Honours class. These assistantships are held by distinguished graduates, generally for a period of three years, and have formed in many cases the training-ground of future professors. It is our object by more adequate endowment of these positions, as well as by the institution of lectureships, to increase the range of philosophical teaching in the university, and so keep pace with the increasing demands of specialism within our subject.

Believe me, yours very faithfully,

ANDREW SETH.

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